Indo-European and Indo-Europeans

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Indo-European and Indo-Europeans

Papers Presented at the Third
Indo-European Conference at the
University of Pennsylvania

Edited by
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Foreword

The studies included here were presented as papers at the Third Indo-European Conference, held at the University of Pennsylvania on April twenty-first through twenty-third, 1966. As at the previous two Indo-European Conferences, held at the University of Texas in 1959 and at the University of California, Los Angeles in 1963, the contributors were invited to treat specific subjects centering about one general theme in Indo-European studies. The theme of this third conference was Indo-European and Indo-Europeans. This topic suggested itself to the organizers principally for two reasons. It is, first, a natural follow-up to the subject of the second Indo-European Conference, which centered about the dialectal distribution of Indo-European languages. The present theme also reflects, we believe, an encouraging fact about what must be considered a true renaissance of Indo-European studies in the United States. This is the renewed interest being shown in the study of Indo-European languages for what information they can yield concerning the culture, social structure, and beliefs of the peoples who spoke them as well as the migrations of these peoples and their contacts with other, non-Indo-European populations. The subjects of the studies as well as their treatments by individual authors speak for themselves in showing the wide fields of interest and the approaches used in dealing with the materials. Previous works used in the present studies also give an encouraging proof of a sense of continuity in Indo-European linguistic scholarship. We are witnessing not an abandonment of older work in favor of newness for its own sake, but a renewed interest in a field which for some time lay dormant and the application to these studies of refinements resulting from new materials and insights.

It is our pleasant duty to acknowledge the generous assistance of the National Science Foundation and the University of Pennsylvania in organizing the Third Indo-European Conference; our special thanks go to Provost David R. Goddard for his warm welcome. We are also grateful to the Haney Foundation for its support and to the University of Pennsylvania Press for their cooperation in presenting these studies to the scholarly public.*

G. C., H. M. H., A. S.

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*For various reasons the following papers could not be included in the published results of this conference: E. Adelaide Hahn, The Supposed Indo-European Construction *in name;* Eric P. Hamp, Armenian *ner;* Paul Kiparsky, Problems of Greek Phonology; and Alexander J. Kerns and Benjamin Schwartz, On the Identification of Indo-European Languages.

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Indo-European and Indo-Europeans

Linguistic Structure as Diacritic Evidence on Proto-Culture

Winfred P. Lehmann

Linguistic paleontology, developed to a fine level at the end of the nineteenth century, has been generally out of favor, at least until the scheduling of this conference. Despite its fascination it was not appreciably advanced after the summary of Otto Schrader in 1883.[1] This forms the basis of the sketch by Holger Pedersen in the last chapter of *Linguistic Science in the Nineteenth Century* (1924). According to R. A. Crossland, 'The picture which O. Schrader drew of the material culture and society of the Indo-Europeans has not been greatly modified by recent research.'[2] Subsequent work in the subject is foreshadowed in Schrader's *Reallexicon der indogermanischen Alterumskunde*;[3] rather than a comprehensive statement on the culture of the Indo-Europeans, individual segments of their culture and language have been discussed in separate encyclopedia articles, in essays, and monographs. Many of these are admirable, such as Professor Benveniste's analysis of the Indo-European words for pig.[4] But there has been no further comprehensive treatment beyond Schrader's; not even Fr. Specht's *Ursprung der indogermanischen Deklination* (Göttingen, 1944) can be so regarded, nor even G. Devoto's recent *Origini Indeuropee* (Florence, 1962). And when we discuss linguistic paleontology in our classes on Indo-European historical linguistics, our remarks are diffident, almost embarrassed. Clearly the linguistic paleontologists had overextended themselves, to the point of elimination.

We could cite a variety of ways in which they overplayed their hand. The one most directly pertinent for linguists is the role they assigned to language in their reconstruction of Indo-European culture. For them language was a primary source. If Proto-Indo-European contains a root *dhey-*, based on reconstruction from Lat. *fingo* 'form, mold', *figulus* 'potter', *figura* 'shape, figure', and the same extended root in Goth. **deigan* 'knead, form from clay', the potter must have been a craftsman in Indo-European society.

Possibly the paleontologists have been berated enough for their free ways with linguistic materials, so that a brief example may be adequate here, one from Pedersen's intriguing paragraph on the Indo-European potter. The Gothic form **deigan* which the usually careful Pedersen cites (*Linguistic Science* 327) is labeled *falsch* by S. Feist.[5] We should probably have been

warned of Pedersen's uncertainty by his qualifiers: 'certainly . . . undoubtedly . . . there can be no doubt' and so on, all in one paragraph on the presumed Indo-European potter and his art. Rather than pursue such details, I would like here to discuss a general principle for paleontology: that language be used only as a diacritic, not as a primary source for reconstruction of early culture.

Viewing language in this way reduces its status among the tools of the paleontologist. But when so used, it should not give rise to the fanciful constructions we find at the beginnings of this century, as in the early Hirt. The basic sources of the paleontologist must be texts and archeological data. Deductions from language must be related to these, examined as commentaries on them, but not used as primary sources.

As an example of a linguistic conclusion valuable for the paleontologist, but as yet unrelated to other findings, we may cite an inference based on the absolute consistency with which the Germanic consonant shift was carried through. From the regularity we may infer that the society speaking Proto-Germanic was close-knit, probably a small, compact group. For if the set of speakers had been in loose contact only, or located over a relatively broad expanse like the High German speakers around the fifth century of our era, we would expect some variation among the Proto-Germanic obstruents after the consonant shift, for example some Germanic initial voiceless stops might correspond to Indo-European voiceless stops. Since there is no such variation, we posit a coherent community. But although we make such an assumption for the Proto-Germanic community, we cannot relate it as yet to any prehistoric culture, among those for which we have archeological evidence in North Europe during the first millennium before our era. The linguistic conclusion is left as an unrelated datum; left in this way it lacks the glamor of elaborations which we find in the illustrated treatises on paleontology. But, although it could be elaborated in a number of directions, without further cultural information it is unwise to manipulate fancifully a solid linguistic datum until we can match it with conclusions from archeological finds.

The example I have given is taken from the general observations of the functioning of language within a culture. We can cite other data in abundance which are useful for the paleontologist, from all levels of language: the semantic, grammatical, and phonological. I recall three well-known examples, one for each of these levels. Each, through a structural aberrancy, gives us a lead toward conclusions about the culture concerned.

An example at the semantic level is Goth. *ulbandus*, ON *ulfalde*, OE *olfende*, OHG *olbanta* 'camel.' Whatever the source of the word in Germanic, its application to the camel rather than the elephant permits the paleontologist to draw some conclusions about the culture of the early Germans. The immediate conclusion is that the Germanic speakers knew the animals only by hearsay. Feist (*Vergleichendes Wörterbuch der gotischen Sprache* 515) gives similar examples, with bibliographical data.

An example at the morphological level is Gk. *hippopótamos*. Again there

may be various explanations.[6] But the aberrant order of the components in an adjectival compound would lead the paleontologist to be cautious about the use of the word for cultural inferences. His most reliable conclusion from the linguistic evidence would be that the Greek word indicates acquaintance with the animal through a different culture.

At the phonological level we might cite OE *scrīfan* 'prescribe', OS *scrīban*, OHG *scrīban* 'write'. Although apparently of a group of verbs which is among the most archaic in the Germanic vocabulary, the initial consonant cluster is aberrant for strong verbs inherited directly from Proto-Germanic and Proto-Indo-European. Accordingly on purely linguistic grounds we would suspect reconstructions of *scrīban* as a term used in the Proto-Germanic community and cited as evidence for one component of the culture of the Proto-Germanic speakers. (To be sure the verb is only attested in the western Germanic dialects, but an apparent nominal derivative; ON *skript*, cf. OE *scrift* 'confession', might mislead us to posit it for an earlier stage of Germanic.) But already the phonotactic evidence deters us from using this word incautiously for linguistic paleontology.

The examples I have given have been largely negative; they have indicated the limits set on the use of the words cited. A linguistic paleontologist might suggest from the linguistic history of the selected items that the Proto-Germanic speakers knew neither writing nor elephants; yet not with absolute certainty, for the words cited may have replaced others.

I would like to illustrate the general principle positively now, centering my discussion about one word in the sphere of the Indo-European social system, Hittite *pankuš*. I regret such narrow restriction, but the field is so complex that further examples could not be treated adequately in short order. Moreover, it will probably be clear that even such restriction, and limitation to texts as primary sources, permits some latitude for the imagination of the linguistic paleontologist.

In dealing with linguistic evidence I follow the admirable procedures of O. Szemerényi.[7] Even these cautious procedures do not permit us to determine without question the words that can be used with assurance in linguistic paleontology. Without going into lengthy detail, I would be cautious about some of Szemerényi's conclusions, as on Lat. *senātus*, pp. 186–7. Though persuasive, the explanation given for it is unique, supported by general linguistic parallels. Szemerényi may have supplied the best explanation which has been given for *senātus*, but it is dangerous to use a tentative explanation for further inferences about early Latin political arrangements. Szemerényi's article also contains comments on *pankuš*; these, based on Ivanov, do not go beyond a linguistic concern.

The term *pankuš* has been widely discussed, as by A. Götze.[8] The most extensive further study is that by V. V. Ivanov, in a number of articles.[9] The term is well attested as an adjective with the meanings 'total, united; general; each', and as a noun with the meaning 'totality'. These meanings, given by Johannes Friedrich, are supplemented by a further gloss with question mark: 'nobility'.[10] This gloss follows Götze, *Kleinasien* 86: 'Der

pankuš, wörtlich "der ganze (Heerbann)", bedeutet mit aller Wahrscheinlichkeit "die Gesamtheit der (dem Kriegshandwerk obliegenden) Adligen".' Friedrich and Götze do not specify further what they mean here by Adel; there is a distinction in the Telepinus text between the members of the king's family and the warriors, whom Götze equates with Adel. As with other Hittite texts, there are Akkadian parallels. But if the Akkadian social system was different from that of the Hittites, we cannot make a one-to-one equation between Akkadian and Hittite words for classes of society. Götze points out (*Kleinasien* 88) that the Hittite conception of royalty is quite different from that of the Orientals in its absence of absolutism and the doctrine of divine right. The difference he ascribes to the social and political system which the Hittites brought with them, and in this conclusion he has been generally followed.[11] It may be noted further that the characterization of the Hittite king as 'primus inter pares', found in Götze (*Kleinasien* 87–8) and subsequently repeated, was given much earlier for the Rigvedic kings.[12] The Hittite texts also indicate that the social and political arrangements of the Hittites were progressively influenced by the Orientals surrounding them.[13] Accordingly, the Hittite texts have furnished us our preliminary information on the meaning of *pankuš*; for further conclusions, especially about the development of *pankuš* as a designation for a social group, we look to linguistic analysis.

Apparently the social designation is secondary. Whether as adjective or noun, the basic meaning of *pankuš* seems to be 'total(ity)'. In dealing with the word semantically, we therefore start from this meaning. We may find some assistance in closer identification by comparing it with other designations for social groups in Indo-European languages. For if the Hittite social organization was brought along from the central settlement area of the Indo-Europeans, other Indo-European languages may reflect similar cultural groupings, or similar linguistic adaptations.

Morphologically and phonologically *pankuš* provides few difficulties. It functions like other *u*-stems in Hittite. There has been some discussion on the primacy of the adjective use over that of the noun, as by Ivanov (1962, 269–70), and Szemerényi (p. 196). But such discussion is of little pertinence for Proto-Indo-European. As Meillet has indicated,[14] there was no inflectional distinction between nouns and adjectives in Proto-Indo-European other than the possibility of inflecting adjectives in more than one gender. To be sure, the distinction between noun and adjective came to be indicated increasingly in the Indo-European dialects, as in Hittite with the appearance of an *a* in all cases of nominal *u*-stems but the nominative, accusative, and instrumental singular. Yet even a Hittite nominal *u*-stem as well established as *ḫeuš* 'rain' has forms with the so-called adjectival *a*. And other *u*-stems besides *pankuš* are used as nouns as well as adjectives. Accordingly the twofold use of *pankuš* as adjective and noun is not remarkable; nor does it give us special leads about the word.

Various scholars have concerned themselves with the etymology of *pankuš*, especially in the past few years. The relationship which Sturtevant

suggested with Skt. *bahu-* 'much', Gk. *pakhús*, 'fat', PIE *bhŋghu-* was cited by Friedrich (*Hethitisches Wörterbuch* p. 157) with a question mark. Ivanov, however, has argued in favor of this relationship (1957, 20–25), and at least in Szemerényi's estimation (*Principles* 196) has established it.

Phonologically the relationship is plausible. And although the proposed relationship in meaning may on the face of it seem reasonable, it is difficult to accept when one compares the uses of *bahu-* in the Rigveda. To be sure, a somewhat similar meaning is acknowledged: 'viel, reichlich, zahlreich'.[15] But even this derived meaning is not yet the collective 'gesamt, vereint; allgemein' of *pankuš*. And since it is not the primary meaning, we would have to assume a further development of meaning from which the uses in Hittite were extended. But even the Vedic phrases in which *bahu-* may be translated 'much, considerable' can be readily interpreted with the meaning 'thick, compact'—e.g. with *vasavyam* 'riches', *ulapa* 'shrubbery' and so on, as also with *jana* '(crowd of) men'. In spite of Ivanov (1960, 794) the phrase *bahúve jánāya* of Rigveda 10.102.8 simply means 'of a thick crowd' and is not similar in meaning to Hittite *pangauuanzi* 'entire'. Even the contrasting *dabhra-*, which in the Rigveda seems to be an antonym, suggests for *bahu-* a primary meaning 'thick,' even 'fat', rather than 'much' and certainly not 'total'. Accordingly, in spite of Ivanov's sensitive interpretation of the Hittite texts, I find it difficult to associate *pankuš*, *panku* with a set of words in other dialects that has the basic meaning 'thick, fat'.

If we do not relate *pankuš* to the Greek and Indic words for 'fat', it would be pleasant to find a different cognate in the other Indo-European languages. This in my opinion Professor Polomé has determined in a fine study on 'The Indo-European Numerial for "five" and Hittite *panku-* "all".'[16] On the basis of a structural analysis of the Indo-European numerals, in which he draws on other numeral systems reflecting a quinary system built around counting on five fingers, Professor Polomé suggested that PIE *penkʷe indi-*cated the total number of fingers of the hand, then 'five'. In *Pratidānam* (*Festschrift F. B. J. Kuiper*, The Hague, 1969) 98–101 he provides additional support for this suggestion from the use of *puntis/puntes* in the Iguvine Tables III. 4, 9, and 10. Here the 'twelve brothers' act in *puntis*. Formerly a tortured explanation was given to account for the supposed activity of the twelve in groups of five. Professor Polomé's suggestion that *puntis/puntes* here means 'as a whole' is much more credible. Since there is no question of the relationship of Umbrian *puntis* to Lat. *quīnque* and the other Indo-European words for 'five', Umbrian preserves evidence for the ancient meaning of **penkʷe*, virtually clinching evidence for its relationship with *pankuš*.

The form is that of the numeral adjective constructed by Szemerényi (*Studies in the Indo-European System of Numerals* 85, 87): **p(e)nkʷos*.

If we assume that the term *pankuš* in its meaning 'total(ity), assembly' is related to 'five/fifth', we should like to find further linguistic evidence that the relationship was recognized by the Indo-European speakers, in keeping with our principle of using linguistic material as a diacritic. Before

examining possible evidence in other dialects, we may recall who actually made up the Hittite *pankuš*. One definition is given by Telepinus when he addresses the *pankuš* as: courtiers, bodyguard, golden-grooms, cup-bearers, tablemen, cooks, scepter-men, overseers of 1000, and chamberlains.[17] Obviously the *pankuš* at the time of Telepinus is not simply the nobility. To be sure we do not expect through the centuries of Hittite materials a static use of the term, nor an unchanging group to which it refers. The complexities of interpretation resulting from partial retention, partial innovation of ancient terms, and presumably of social groups are well illustrated in Palmer's *Achaeans and Indo-Europeans*. Later *pankuš* may well have specifically designated the warriers. Yet earlier it apparently referred to a more general group closely associated with the king.[18] In contrast with Götze's interpretation on its makeup as the 'Gesamtheit der Adligen', (*Kleinasien* 86) for Sommer the function of the *pankuš* is that it is consulted by the king when the upper classes commit a religious offense (*HAB* 211). Whatever the details, neither for Hattusilis (ca. 1650 B.C.) nor for Telepinus (ca. 1525 B.C.) does it refer to the warriors alone, rather to a broad spectrum of the 'people'.

In the Rigveda there are remarkable references to 'fives' of the people as a whole: *páñca jánās* 'five races', *páñca carṣaṇís*, *páñca kṛṣṭayas*. There is a score of such passages, of which we may cite the following with *mánuṣa*:

VIII. 9. 2. (629.2) yád antárikṣe yád diví yát páñca mānuṣān ánu
nṛmṇáṁ tád dhattam aśvinā
Aśvins, arrange whatever divine force exists in the air, in the heavens, and among the five peoples.

The Rigveda is not transparent in providing a reason which might clarify references to all the peoples as five; no such reason is suggested even when the five peoples are named, e.g. I. 108. 8. Yet we may draw some inferences from I. 108, which is a prayer to Indra and Agni, asking them to approach and drink soma, wherever they might be, whether in the house of a Brahman, or a rājanya, or among one of the five peoples. Because of this contrast between the three groups, we might tentatively suggest that the 'totality of peoples' are those apart from the priests and the kingly group.

It would be pleasant to be able to determine the basis for the names of the five peoples, to attempt to arrive at an explanation for the five names. But to the present we can do little more than cite the Vedic passage. Ludwig's statements, for example (*Der Rigveda* 3.167–72), merely provide a general commentary on the passages in which the 'five peoples' are mentioned without giving us information on the peoples themselves. Stanzas 7 and 8 of the Vedic hymn in which the names are mentioned simply provide the basic prayer, and the situation:

7 Whether you, Indra and Agni, are enjoying yourselves in your own homes, whether with a Brahman or a Kṣatriya, please come here, you bulls, and drink from the pressed soma.

8 Whether you, Indra and Agni, are among the Yadu's, the Turvaśa's, among the Druhyu's, the Anu's, or the Puru's, please come here, you bulls, and drink from the pressed soma.

Stanzas 9 and 10 continue the requested summons, whether Indra and Agni are in the highest, the middle or the nearest segment of the earth; stanza 11 summons them, whether they are in the heavens, on the earth, in the mountains, on the plains, in the waters. These stanzas complete the possible locations of the two gods, but do not relate the locations to the five peoples—here called *mā́nuṣās*, though presumably the *páñca jánās* referred to elsewhere in the *Rigveda* and the *Aitareya Brāhmaṇa*—where they are said to be sons of Yayati.

I assume that these heroes are the eponymous progenitors of the five *jánās: Pūru, Yadu, Turvaśu, Anu* and *Druhyu*. Little more can be inferred about them. It may be of some relevance to note that they are names formed with *u*-stems, and to recall the frequently cited fondness for *u*-stems in Indo-European words which have a religious meaning. But apart from this remote relationship, I have nothing to suggest concerning these names which might illuminate cultic arrangements for the Indo-Europeans.

The names themselves occur elsewhere in the early Indic texts; see F. E. Pargiter (*Ancient Indian Historical Tradition*, 1922, especially 292ff) who draws on Purāṇic and epic, as well as Vedic, tradition. All the names but *Yadu* occur elsewhere in the Rigveda. In explaining them, handbooks comment that they are the names of tribal heroes, and attempt etymologies for all but *Yadu*. *Pūru* is related to *puruṣa* 'human being', *Turva(śu)* to *tūrvati* 'conquers'. *Turvaśu* and *Yadu* seem to be associated closely, but the association gives us no further information on these two names. *Anu* is often related to the negative particle, and assumed to mean 'non-Aryan', but this assumption is strained, from the point of view of nominal formation. *Druhyu* may be related to *druhyati*, OHG *triogan* 'deceive', ON *draugr* 'ghost'; this set has tentatively been related further to ON *dvergr*, through metathesis of the resonants (cf. WP 1. 984). If so, one of the names for mythical groups in the *Edda* would have a similar origin to that of one of the five peoples. Whatever their relationships, the apparent source of some of the five names does not conflict with possible names for clan heroes.

Of the system of clans in early Indian times we unfortunately know little. John Brough, in the introduction to his translation of the Gotra-Pravara-Mañjari of Puruṣottama-Paṇḍita (*The Early Brahmanical System of Gotra and Pravara*, 1953) reaffirms his earlier suggestion 'that the "hymn-families" of the Rigveda are the direct ancestors of the main gotras in the Sūtra accounts' p. xiii. We may make the further suggestion that the eponymous ancestors of the five tribes are founders of clans. Whether we should assume totemism of these clans is unclear; Brough is hesitant to do so, p. xvi, and the names of the five peoples seem to provide no evidence for it.

In other Indo-European languages there are tantalizing references to groups of five. The most ancient of these is in the Hittite laws, where five sets of artisans are named: carpenters, blacksmiths, weavers, leatherworkers and fullers. 'Wenn jemand einen Jungen zum Ausbilden gibt, entweder als Zimmermann oder als Schmied, als [Web]er oder als Lederarbeiter oder als Walker, . . . ' Johannes Friedrich (*Die Hethitischen Gesetze* 1959, 87). It may be possible that these five sets reflect a substitution of

trades for an earlier division into clans. A later passage in Herodotus may also reflect the fivefold division of the artisans; Herodotus tells us (Book One, section 93, *Herodoti Historiarum Libri IX*, ed. H. R. Dietsch. Leipzig, 1906) that the tomb of Alyattes was put up by the tradesmen, craftsmen, and prostitutes; on the top of the tomb they placed five pillars of stone with inscriptions telling the amount of work by each class. It may also be his knowledge of such a society which causes him to distinguish in Egypt five groups of classes besides the priests and warriors—seven *genea* in all, (Book 2, section 164). These groups would be late, in comparison with the Indo-European 'peoples', and would have been held together by economic ties. There is a possibility that we may propose a different binding force for the *páñca jánās* or *mánuṣās*.

For in Rigveda VIII.9.2, they are indicated to be possessors of *nṛmṇám*.

If we follow F. B. J. Kuiper's intriguing suggestion,[19] that we should reconstruct IE **h²ner* 'vitality', we may interpret *nṛmṇám* as a derivative of this. Like Greek **áner*, **ánar* it may be further glossed 'vital energy—mana—divine potency', cf. Kuiper 9, 12, 14. If the five peoples have this, we have further support for the suggestion that they are groups bound together by a special force, groups comparable to 'brotherhoods, clans'. Ivanov has already proposed this meaning for *pankuš* in early Hittite, 1960, 795–7.

Examining the Hittite *pankuš* with this meaning in the early texts, we recall Sommer's comment that a guilty member of the *pankuš* harms the entire *pankuš* through his crime, *HAB* 186; this is a widespread notion for a society composed of clans. Moreover, the tremendous concern of Telepinus with infractions of language and their effect on the *pankuš* reflect oral taboos well known of clan organizations. Some of the 'archaic religious conceptions' may be reflected in linguistic patterns, such as the use of **dhē-*, Hitt. *tē-* for cultic statements; see Sommer, *HAB* 15–17: HAB III. 60–61 na-aš-ma ut[-tar k]u-iš-mi [ku]-it-ki te-iz-zi . . . 'Bemerkst du bei einem ein Vergehen, daß vor der Gottheit einer sündigt oder [e]iner [ir]gend-welches (frevlerisches) Wo[rt] ausspricht, so befrage jeweils (den pa-an-ku-un).'

On the other hand, one of the desired gifts of the gods is *innarauṷatar*, as apparently through the Ritual of Anniwiyanis; (see Sturtevant-Bechtel, *Chrestomathy*, 100–26). According to A. Kammenhuber, *innar-* means '(Lebens) Kraft, hoheitliche Macht', is a quality related to the royal house and cultic functions, and if present in the royal family benefits the entire country.[20] Apparently we have here too a reflection of PIE *hner-* 'mana', one of the attributes of the clan. The purposes of this ritual, and of other rituals of which we have evidence such as that at Iguvium, as well as much of the detail are still unclear. We may recall that in one of the perplexing passages of the difficult Iguvine tablets—the beginning of II b, there is a further instance of a cognate of 'five': *tekvias*:*fameřias*:*pumpeřias*:*XII*. Without discussing the relationship of the four successive items, I would like to point to the possibility of interpreting *pumpeřias* as a religious unit

of some kind, possibly 'clans' in the context of the other units: *tekvias* 'phratries (?)'—'families'—and the group of twelve brothers.[21] Whatever the relationship of the terms, five proper names follow immediately with *-t-* suffix, five with *-n-* suffix. Until we know more Etruscan, any identification of the *n*-suffixed names is out of the question. But, since each *n*-suffixed name is given directly after one with *t*-suffix, it is conceivable that they refer to different functions of the *t*-suffixed names.

Finally, we may recall other words for the 'five peoples' of the Rigveda: *carṣaṇís* and *kṛṣṭís*. M. Mayrhofer[22] equates them, admitting little else than a common translation 'people, peoples', though he favors relationship with *karṣati* 'plough'. Meillet's comparison with Gk. *télos* 'host' is certainly preferable, with derivation from PIE *k*^{w}*el-* 'move about, dwell'. If so, the early meaning is not 'ploughman' but rather 'active (men)', as in Skt. *abhi-cara-* 'companion', Gk. *oio-pólos* 'sheep-herd', etc. A Hittite derivative of this *k*^{w}*el-* may be *kaluti-* 'group', which in KUB VI. 3. 12 corresponds to *pangaṷaš* of other texts. If *télos* is a reflex of an earlier term for 'people', even 'clan', we may have another survival of this type of organization in the word *teléstas* 'an initiated person; priest'; see Liddell and Scott (*Greek-English Dictionary* 2. 1770–71). The interpretation is also supported by related forms, such as *teletḗ* 'sacred office'—and in the plural 'festival accompanied by mystic rites'.

Examination of a linguistic item, subsequently others, has in this way provided a diacritic to Hittite texts and those in other Indo-European languages. It has also led to consideration of cultic terminology which has survived in some of our oldest Indo-European materials. Clearly this discussion has touched on only a few of the linguistic retentions in our texts, only those in Hittite, Indic, Greek and Umbrian, and even only superficially on these.[23] I suggest that the insights of Ivanov, Polomé, Kuiper, and Kammenhuber be further pursued, primarily with reference to the texts themselves rather than the terminology. The Ritual of Anniwiyanis, the Iguvine Tablets, some of the Vedic hymns, and the scanty reflexes in other early dialects should be pursued for their cultic terminology. Additional analysis of the texts, with further insights derived from linguistic relationships, may enable us to construct more of the Indo-European religious practices than has been furnished in previous attention to the culture of the Indo-Europeans.

Notes

[1] *Sprachvergleichung und Urgeschichte* (Jena, 1906–07) 3r ed. Pedersen's *Linguistic Science in the Nineteenth Century* was first published in 1924; it was translated by John W. Spargo (Cambridge, 1931), and recently reprinted without change, as *The Discovery of Language* (Bloomington).

[2] Indo-European origins: The linguistic evidence, *Past and Present* 12 (1957) 16–46, p. 22.

[3] 1901; 2nd ed. by A. Nehring (Berlin and Leipzig, 1917–29).

[4] Noms d'animaux en indo-européen. I Le nom du porc. *BSL* 45 (1949) 74–103.

[5] *Vergleichendes Wörterbuch der gotischen Sprache* (Leiden, 1939) 118; Feist permits only the hypothetical **digan.*

[6] See Frisk, *Griechisches etymologisches Wörterbuch* (Heidelberg, 1954–) 1.733.

[7] Principles of etymological research in the Indo-European languages, *Innsbrucker Beiträge zur Kulturwissenschaft*. Sonderheft 15. 175–212 (Innsbruck, 1962).

[8] *Kleinasien, Handbuch der Altertumswissenschaft* III.1.3.31 (München, 1957) 2nd ed., 86–8.

[9] Sotsial'naja organizatsija indoevropeiskix plemen po lingvisticheskim dannym, *Vestnik istorii mirovoi kul'tury* 1957: 1. 43–52; Proisxozdenije i istorija xettskogo termina panku sobranije. *Vestnik Drevnej Istorii* 1957: 4. 19–36; 1958: 1.3–15; L'organisation sociale des tribus indo-européennes d'après les données linguistiques, *Cahiers d'histoire mondiale* 5.4 (1960) 789–800; Hittite word-formation in the light of historical comparative linguistics, *Trudy* 25. *Meždunarodnogo kongressu vostokovedov* (Moscow, 1962) I. 268–75.

[10] *Hethitisches Wörterbuch* (Heidelberg, 1952) 157. He gives the following meanings: (1) Adj. 'gesamt, vereint; allgemein;—ein jeder'. . . . 2) Subst. c. 'Gesamtheit; —Adelsgemeinschaft(?)'.

[11] See L. R. Palmer, *Achaeans and Indo-Europeans* (Oxford, 1955) 14.

[12] See A. Ludwig, *Der Rigveda* (Prague, Leipzig, 1876–1883) 3. 251.

[13] See for example on the changing role of the *pankuš* F. Sommer, *Hethiter und Hethitisch* (Stuttgart, 1947) 28.

[14] *Introduction à l'étude comparative des langues indo-européennes* (Paris, 1937) 8th ed. 252: Les adjectifs n'ont pas de flexion différente de celle des substantifs; le seul trait qui les caractérise est la distinction des genres.

[15] This is the fourth meaning given by Hermann Grassmann, *Wörterbuch zum Rigveda* (Wiesbaden, 1955). Third, unchanged edition. Cols. 902–03; the first meaning is 'dicht'; the second and third are related to it, in special uses for quiver-'densely packed quiver' and for the arrows in the quiver. It is clear from the nouns with which *bahu-* is used that the fourth meaning is also derived from 'thick'.

[16] He presented his findings at the meeting of the American Oriental Society in 1963, and these are to be published in *Orbis*; now he has extended his analysis in a comment on O. Szemerényi's Studies in the Indo-European System of Numerals (Heidelberg, 1960) *RBPhH* 44 (1966) 229–233. Chronique No. 65. Les numéraux indo-européens.

[17] O. R. Gurney, *The Hittites* (Pelican, A-259, 1952) 68; for the text itself, see E. H. Sturtevant and G. Bechtel, *A Hittite Chrestomathy* (Philadelphia, 1935) 180, 192–93; the translation on p. 193 differs slightly from Gurney's.

[18] See F. Sommer, *Die hethitisch-akkadische Bilingue des Ḫattušili* I (Labarna II.) (München, 1938) 186, 211.

[19] Νώροπι Χαλκῷ *MAWNed*, NR 14.5 (Amsterdam, 1951) 17.

[20] Hethitisch innarauuatar, LÚKALA-tar und Verwandtes, *Münchener Studien zur Sprachwissenschaft* 3 (1958) 27–44.

[21] For earlier interpretations, see J. W. Poultney, *The Bronze Tables of Iguvium* (Baltimore, 1959) 190–93, who translates: 'The *decuviae* and the quincurial families are twelve' though he also discusses the problems at length, here and elsewhere, e.g. p. 199.

[22] *A Concise Etymological Sanskrit Dictionary* (Heidelberg, 1953) 1. 263–64, 379–80.

[23] I am grateful to the participants in the conference who commented on this paper. I have also profited from discussions with Professor Polomé, particularly on his etymology of 'five', from comments by Professor Cowgill, who with his usual restraint suggested positions where caution might be applied, and to Raja Rao, for his information on Indic culture. D. D. Kosambi, The Vedic 'Five Tribes'. *JAOS* 87 (1967) 33–39, has discussed the references to the pañca janās in the Rigveda and other Sanskrit works with the aim of accounting for 'all the divergent features of the epic tradition, besides explaining what became of the Five Tribes.' His article accordingly furnished a summary of the Sanskrit references, and relates their subsequent history to developments in India; it does not attempt an interpretation of the origin of the phrase in relation to early Indic culture, as is done here.

Proto-Indo-European Trees[1]

Paul Friedrich

I. Introduction

This short study treats one small portion of the language and culture system of the speakers of Proto-Indo-European dialects, who are assumed to have been scattered in a broad band over the steppe, forests, and foothills between the western Caspian and the Carpathians, roughly during the fourth and the first centuries of the third millennium B.C.

The general hypothesis to be presented and supported consists of three parts. First, that the Proto-Indo-Europeans recognized and named at least 18 kinds of trees; the combination of name and referent will hereafter be referred to as 'unit.' Second, that the Proto-Indo-European language or, in many cases, large sections of it, contained at least 27 names for trees which are attested in varying ways in the daughter languages, particularly Italic, Germanic, Baltic, and Slavic, and, to a lesser degree, Celtic, Greek, and Iranian. Third, that the 27 tree names refer to classes of trees that in the main correspond to genera such as the birch (*Betulus*), but are in other cases limited to single species such as the 'Scotch pine' (*Pinus sylvestris*); some classes cross-cut the familiar categories of the English language, or of Linnaean botany, as when PIE *Ker-n-* is said to include both the wild cherry (*Prunus padus*) and the cornel cherry (*Cornus mas*).

The tree names, and the meanings attached to them, and the botanical realities, are assumed to constitute a system of interdependencies that can only be discovered and interpreted by conjoining three approaches: (1) linguistics and philology, (2) botany (particularly paleobotany), and (3) the relevant aspects of ethnography, ethnology, and archaeology. Let us consider these component systems and their conjunctive synthesis, as a methodological preface to a statement about the arborial aspect of Proto-Indo-European culture.

First, the phonology and grammar of the Indo-European languages and of Proto-Indo-European occupy an important position within structural and historical comparative linguistics. For over a century single words, roots, affixes, and phonological units have been the subject of articles, and single dialects the subject of major monographs; this has resulted in a rigor and intensity with few parallels among the humane sciences, and a degree of factual control and analytical insight not shared by the linguistics of any other language family. Accordingly, about half the discussion that follows is devoted to citing and comparing cognates, and to determining the phonological shape or at least the probability of the presence of the Proto-Indo-

European tree names, with an awareness that "les rapprochements valent seulement dans la mesure où ils sont soumis à des règles strictes" (Meillet 1937:41). The array of etymologized roots and the relevant apparatus for their phonological and morphological interpretation, may be referred to as the *linguistic system*.[2]

Second, just as its shape is an abstraction from the tabulated phonological correspondences, so the denotation of a proto-form is assumed to be an abstraction from the tabulated semantic correspondences. But how is this abstracted denotation to be stated? On the one hand, copious materials lie in condensed listings and parenthetical or telegraphic comments in etymological dictionaries and textual concordances. On the other hand, full-scale treatment of even a single word can run to article or book length if it is to take account of all the logical ramifications and the previous scholarship (e.g. Eilers and Mayrhofer on the beech). Distinct from both the dictionary entry and the elaborate exposition are the short summaries below, which draw on the extant comparative philology and textual analysis to establish the probable botanical referent of each tree name.[3] But etymology is only half of the second analytical system.

The traditional 'science of etymology', concerned with the unique histories of words and word families, needs to be integrated with linguistic ethnography or 'structural anthropology'. These newer sciences are concerned with the contrasts and complementation between morphemes and words in domains, and with the sets of distinctive features which underlie such domains. There is a need to conceive of semantic phenomena, not as lists and inventories, but as structurally differentiated, interdependent, and significantly covarying units (e.g. Benveniste 1949). For this philological-ethnographic framework and its results I would use the term *semantic system*.[4]

Third and last, the reconstructed lexicon may be related to certain data and systems that are external and neither linguistic nor semantic. The external *data* may range from archaeological site reports to paleobotanical pollen diagrams; the external *systems* may range from the general concept of a Neolithic technology to, as in the present case, the arboreal ecosystem of eastern Europe—a set of trees related to each other, and to other biota, and to the climate, the soil, and space, and time, as part of a 'trophic-dynamic equilibrium' (Lindeman 1942).

One's control of such botanical and ecological variables must crucially affect the adequacy of one's glosses of the PIE tree names. Cultural reconstruction, and proto-ethno-science in particular, must take account of the up-to-date information from the relevant humane and natural sciences—in this case from the relevant paleoscience.[5] The process of tying the results of linguistics and semantics 'outwards' might be called disambiguation via contextualization. For the entire field of nonlinguistic and non-philological data and concepts I would use the term *external system*.

Let me illustrate the conjunction of three analytical systems. I would regard a PIE tree category as secure if potential cognates in four or more

stocks, or at least two noncontiguous stocks, conform in large degree to three criteria:

(1a) that of established 'phonological laws' (e.g. initial /p-/ in Russian corresponding to initial /ø/ in Irish);

(1b) that of established patterns of semantic change and covariation (e.g. the metonymical shift from 'ash' to 'ash or spear' represented by Homeric *oksúe:* ; Cunliffe 1963:295), and

(2) if the reconstructed form refers to a botanical type that is palynologically or otherwise physically attested for the Proto-Indo-European homeland during the Atlantic.

However, I would also posit a tentative Proto-Indo-European unit in weaker cases, as when there is strong linguistic and semantic evidence but no botanical evidence (e.g. the yew), or vice versa (e.g. the linden).

The conjunctive approach develops from the principle that the science of linguistics be concerned with value systems of sound in natural language, and with value systems of meaning in human culture (including its relation to the natural environment), and with the interconnections and interpenetrations between these two levels. The conjunctive approach, by implication, rests less heavily on tight or 'stringent' proofs using only one line of evidence, as against establishing degrees of probability on the basis of all relevant and available evidence. By studying the diverse facts, evidence, and models of reality, and the intersections and mutual reinforcements between them, the conjunctive approach reduces the indeterminacy or 'nonuniqueness' of narrowly based solutions—particularly of those based on purely formal operations and purely internal textual analysis. The conjunctive approach may eventually lead us to rewrite many dictionary entries and—what is more interesting—to develop an adequate theory of Proto-Indo-European culture, that is, of the system of perceptual and conceptual values shared and transmitted by the members of this primitive neolithic society.

II. Eighteen Arboreal Units

The biological concept of 'succession' (e.g. Woodbury 1954) refers to the systematic way members of a biotic community—including, of course, arboreal communities—succeed to each other and realign over space and through time. Beneath this rubric, geological succession involves long time spans of thousands of years and is connected with such factors as climatic change; ecological succession, on the other hand, involves scores or hundreds of years and is concerned, for example, with replacement by ecological pioneers and later species after floods, forest fires, and so forth. At any given time some species or genera may be dominant, and climax forests may form, sometimes only involving a single species; simultaneously, other types of trees, though less well represented, will be present as minority members or in limited ecological niches.

The 18 tree units which I have reconstructed for Proto-Indo-European are discussed below in the approximate order of their geological succes-

sion in eastern Europe, from (1) the Boreal (about 7000 to 5500 B.C.), to (2) the warmer and moister Atlantic (about 5500 to 3000), to (3) the more continental and, some would argue, more arid sub-Boreal (3000 to 800 B.C.). Special attention is attached to the second of these periods—the Atlantic—which coincides roughly with the millennia of Proto-Indo-European speech unity.

1. THE BIRCH. The reflexes of the PIE birch name are distributed through six stocks. They are particularly widespread in Germanic, Baltic, and Slavic—for example, Old Prussian *berse* and Russian *berëza*—but also run eastward to Iranian (Ossetian) *bärz* (Abaev 1958:253) and the *bhu:rjá-* of the (Indic) Black Yajurveda.[6] *BherHǵo-* has long been accepted as part of the Proto-Indo-European inventory; it is sometimes cited in discussions of laryngeal theory (Lehmann 1953:53, has *bherH-g-*), and has been classed as an archaic feminine o-stem (Meillet 1923:197); the correspondences for the initial voiced aspirate are completely regular.

The cold-hardy, fast-growing birch entered early with the pine, and one species in particular (*Betulus alba*) was dominant through much of the Boreal, although it receded rapidly in the early Atlantic before the advance of a principal rival, the hazel. *Betulus* has persisted as an ecological pioneer, as a minority member of European forests, including those of European Russia, and as a basic arboreal component in the Crimean and Caucasus areas (Firbas 1949:282).

2. THE SCOTCH PINE. A salient fact about the Proto-Indo-European arboreal system is the underdifferentiation of the conifers.

Pre-PIE *pV-* is probably the origin of three words. The first *pyt(w)-*, is reflected in four stocks, including Greek, Latin (*pi:nus*), Albanian *pišė* (from *pi:t-s-ia*; G. Meyer 1891:340), and Old Indic *pi:tu-da:ru.*[7] The other words, with k-extensions, are reflected in six stocks, including Slavic, Greek, and Latin, and may be illustrated by Lithuanian *pušis*, Old High German *fiuhta*, and Middle Irish *ochtach* (Book of Lecan; Stokes 1895:73). However, the meanings of these various cognates embrace all three conifers (pine, spruce, and fir), and typical products such as tar and resin.

The semantic facts—which otherwise would lead to positing an arbitrary combination of meanings or 'conifer' in general, or some yet vaguer referent —are resolved by the paleobotanical evidence: the pollen of the fir (*Abies*), although amply produced and readily identifiable, is practically absent from the Proto-Indo-European homeland during the Atlantic and, with one exception on the southern Volga, is limited to the high Caucasus (*A. Nordmannia*; Walter 1954:161), and to such northern peripheries as Poland (the silver fir) and Siberia (the Siberian fir). The common spruce (*Picea excelsa*) was also continuously absent in the Ukraine, although during the Middle and Late Atlantic it spread into marginal areas such as Armenia and the Pripet-Desna. On the other hand, one species of pine, the Scotch pine (*Pinus sylvestris*) has been widely present in and around the Ukraine since the pre-Boreal, and was prominent during the Boreal, although forced back somewhat by the hardwoods during the Atlantic. With the exception of some peripheral dialects, therefore, pre-PIE *pV-* denoted 'Scotch pine.'[8]

3. THE JUNIPERS AND CEDARS. An early PIE form evolved into Armenian *elewin*, 'cedar', which corresponds well phonologically with White Russian *jelenets*, 'juniper', and Proto-Slavic *jalovItsI* (Vasmer 1950–58: 3.488; Lidén 1905–56: 491–2). The Armenian and Slavic words may be related—in a way that remains to be satisfactorily explained—to forms in three stocks that refer to two different conifers, namely the fir and the spruce: Greek *elάte*: and Proto-Balto-Slavic *edla*: or *edli*: (Hübschmann 1897: 442, described the separation of the /-dl-/ of the latter from the /-l-/ of Armenian as 'insuperable').

The relation of the Baltic and Slavic forms to the Italo-Celtic *edlo-*, as reflected in Gaulish *odocos*, has been evaluated by Benveniste (1955) as a case of 'Indo-European homonymy'; in support of his position, I would emphasize that the Italo-Celtic *edlo-* denoted, not an aromatic evergreen, but the dwarf-elder, a white-flowered, deciduous, and smelly bush.[9]

Not long after the glaciers the juniper and cedar entered together with the pine. The pine is hard to distinguish palynologically from the cedar. The juniper, on the other hand, is widely attested in east European sites of the Atlantic period. Both trees probably occurred throughout the homeland area, usually as an understory beneath the conifers, or as pioneers on dry or barren ground. They resemble each other closely—as in the blue berries and shreddy bark—and are in fact often cross-labeled or confused in folk taxonomies, such as those of Old Slavic, and American English. I would conclude that a late and dialectal PIE *el-w/n-* denoted the class of 'juniper-cedar'.

4. THE ASPENS AND POPLARS. PIE *asp-* has possible cognates in Greek *áspris*, 'oak', and even *aspís*, 'shield', but they are improbable (Frisk 1954:169)—particularly because of phonological objections regarding the first vowel. A second set of possible cognates occurs in Indic and Iranian, but their denotations—of oar, shovel, scapula, and above all, a sacrificial instrument (Vedic *sphyá*; Böhtlingk-Roth VII:1381; Janert 1964)—raise grave and still unsolved semantic and ethnological questions. Otherwise, *asp-* is attested in the three north-central stocks: Baltic, Germanic, and Slavic, as in Proto-Slavic *opsa* (Vasmer 1950–58:282). *Asp-* was probably an areal term shared by certain early Indo-European tribes together with certain Ural-Altaic-speaking peoples of eastern European Russia and southwestern Siberia (e.g. Modern Altai *apsak*, Kumandu *aspak*; Hoops 1905:122–4).[10]

Botanically, species of *Populus* had been present since the glaciers, and persisted as ecological pioneers and minority members during the Atlantic; the 'quaking aspen' (*P. tremula*), prominent in the Baltic and much of European Russia, and *the* tree of trees in southwestern Siberia, was probably the referent of *asp-*.

5. THE WILLOW. The cognates for *sVlyk-* include Latin *salix*, Old Irish (genitive singular) *sailech*, Greek *helikó:n*, and Old High German *salah(a)*. They are probably related to a second set with the same willow referent that contains Germanic *wilge* and Greek *welikó:n* (Frisk 1954:494); the two sets reflect a PIE alternation of initial s-/w- (Meillet 1937:171–2). *S/wVlyk-* may have cognates in Hittite *ɥellu*, 'meadow' (J. Friedrich 1952:251), or

in *ɣelku*, 'grass' (Pritchard 1955:354); the two unlikely candidates involve improbable semantic shifts. A second willow word (*wyt-*) is attested in nine stocks, as illustrated by Lithuanian *vytìs* (Fraenkel 1962:1268) and Avestan *vae:tay* (Bartholomae 1904:1314).

PIE *s/wVlyk-* has been derived from *selk-*, 'wind, twist.' *Wyt-* may be a derivation from a verbal root, *wy̆-*, 'bend, twist'; 'en réalité, il s'agit de la branche flexible qui peut être utilisée de toutes sortes de manières' (Meillet 1937:397). A third willow word (*wrb-*), reflected in Slavic, Baltic, and Latin, may stem from another root for 'turn, twist' (*wer-*). Such derivations are consonant with the probability that willow shoots were woven in Proto-Indo-European technology.

Wyt-, *wrb-*, and *s/wVlyk-* probably served to differentiate in some way between two obvious physical classes of this moisture-loving and most diversified genus (*Salix*): the tree willows, and the osiers, both of which, as evidenced by wood and bark deposits, were present all over the USSR during the relevant millennia (Nejshtadt 1957:266), particularly along the river banks favored by the Proto-Indo-Europeans themselves.

6. The Apple. While palynological evidence is virtually nil, because of the perishability of its pollen, the common wild apple has probably been present since the Boreal or early Atlantic, and is a usual minority member of mixed hardwood forests in Europe and Anatolia; the common cultivated apple was domesticated in the highland regions just south of the Caucasus (and the Proto-Indo-European homeland); apples occur in Neolithic sites of Switzerland and elsewhere (Firbas 1949:189); and finally, the apple—a sort of northern grape—was demonstrably of great dietary, social, and even mythological significance to early Indo-European groups (see, for example, Tacitus' *Germania* 23); these and similar facts make it reasonable to assume that the Proto-Indo-Europeans probably had an apple term.

The Oscan town name, *Abella*, meaning 'rich in apples' (*Aeneid* 7:40), presumably goes back to an early form that was brought in from the north with the Italic invasions. Otherwise, four 'northern' stocks display numerous cognates denoting apple or apple tree (e.g. early Celtic *ablu*, early Baltic *a:bo:l*), which resemble each other sufficiently in form and meaning to have led Meillet to affirm the existence of a term 'to designate the "apple" of the ancient people of Indo-European speech in northern Europe' (Ernout-Meillet 1951,1:15). Most Indo-Europeanists are disturbed by the 'formal anomalies' (the medial /-b-/, the *l*-stem, the uncertainty of the vocalism), and would prefer to regard *abVl-* as ultimately of non-Indo-European provenience (Meillet 1937:398); some think it was borrowed but once—into Celtic—whence it diffused to Germanic, and then Baltic, Slavic, and Italic).

The second apple term has reflexes in five stocks, although only Greek *me:lon* is certainly not a borrowing; Latin *ma:lum*, like the Albanian forms (Tagliavini 1937:195) may well have come in from Greek, possibly in asso-

ciation with a cultivated variety of the tree. Both Tocharian *malañ*, 'cheeks', and Anatolian (Hittite) *maḫla-*, 'grape' (Ehelolf 1933) have been widely rejected. Nevertheless, both forms correspond well phonologically, and imply shifts that are semantically plausible—note, for example, the standardness of the apple : cheek metaphor in both Greek and Albanian, and also the fact that both grapes and honey apples (Latin *melime*:*lum*) are rendered into juice and pulp. A southern PIE *ma*:*lo-*, with reflexes at least in Greek and Anatolian, seems cogent.

The probability is that both *abVl-* and *ma*:*lo-* were genetically independent, and regionally limited to the north and south, and it is possible that both were ultimately of non-Indo-European provenience.

7. The Maples. The first, *akVrno-*, is reflected in three stocks: Latin *acer*, Greek *ákarna/ákastos* (Frisk 1954, 1:151), and in Germanic—Osthoff posited a Gothic **e*:*haurns*. The Rigveda contains one instance of *akráḥ* (X, 77, 2), which matches well phonologically, but is difficult to defend semantically (usually translated as 'banner, wall, fence'; Brunnhofer 1901:108–9).

The second maple word, *klen-*, is supported by a tier of four northern stocks that includes Baltic, and may be illustrated by Welsh *kelyn* and Russian *klën*. Old High German *li*:*n-boum*, 'maple' (from Proto-Germanic *hluniz*) may have a cognate in the parallel construction of Greek (Theophrastus *klinótrokhos*, 'sweet bay or Olympian maple'. Thus, Germanic shows reflexes of both the maple terms.

Acer pollen, scantily produced and rapidly deteriorating, is characteristically under-represented in the records, and the history of the genus is not well understood. However, at least three species—the Norway, the sycamore, and the common maple—have probably been present in central Europe since the early Atlantic, and there was probably already a relatively great subgeneric differentiation in the Caucasus and Bashkir areas. *Acer* pollen has not been found in the Ukrainian deposits of these millennia (Frenzel 1960:387–97), but the common maple (*A. campestris*) was probably present.

The botanical and linguistic evidence on the maple may be correlated in the form of two complementary hypotheses. First, the two Medieval Germanic and Proto-Germanic forms presumably symbolized a folk-botanical contrast between two physical classes of maple, a contrast which may stem from late Proto-Indo-European, when some tribes were already well west of the homeland. Second, both the late Proto-Indo-European maple roots may go back to a single root—tentatively *kl-n-*, used by the early or pre-PIE for *Acer campestris*.

8. The Alder. The old PIE feminine o-stem, *Vlyso-*, is reflected in six stocks, including Baltic and Latin (*alnus*). Gothic **alisa* was preserved in Spanish *aliso* (Kluge 1963:172), and corresponds with Proto-Slavic *olIkha/jelIkha* (Vasmer 1950–58:2.226); before a vowel and after four sounds, including /y/, PIE /s/ goes to Slavic /kh/. More questionable as

cognates are the Celtic "Alder God," *Alisanus* (from Proto-Celtic *alisa*; Walde-Hofmann 1938:31), 'Place of Alders'.

Like the willow, *Alnus* pioneers vigorously in moist areas, and, like the hazel, produces large quantities of readily identifiable and long-lasting pollen. There are four regionally limited species—the gray, bearded, black, and mountain—of which three can be distinguished palynologically. The gray and bearded alder have been present in and around the Caucasus since the early Holocene, when the latter was outranked only by the chestnut (Nejshtadt 1957:293). During the Atlantic, the alder—probably the black, and possibly the gray—migrated into the Ukraine from the north and attained maxima of 24–58 % (Frenzel 1960:387). During the Middle Atlantic *Alnus*, probably including the mountain species, moved westward into Germany. The subsequent, rapid retreat in the sub-Boreal was probably due to a combination of aridity and the advance of the spruce, which creates deep shade in areas of moist soil, and also to man, who cleared out favored alder niches such as the banks of streams and rivers. (Gimbutas discusses this Proto-Indo-European pattern in some of her articles.)

9. THE HAZEL. Cognates in three stocks denote the hazel and agree well in form—for example, Proto-Celtic *kosl* (Pedersen 1909, 1:184) and Proto-Germanic *kos(r)lo-* (Kluge 1963:291). The western Indo-European masculine o-stem, *kos(V)lo-*, may be related to the phonologically matching Lithuanian *kasulas*, 'hunter's spear for hares and small game' (Niedermann 1902:97–8; Pokorny 1959, 1:616), but contemporary Balticists do not think so. Also doubtful is the Slavic cognate *leško* (Machek 1950:154), involving metathesis of /l/ and /k/ through contamination from the associated word for 'nut'.

Botanically, *Corylus*, like the alder, coexisted with the pine and birch for millennia before advancing sensationally and often dominating during the early Atlantic ('die Haselzeit'). This was a prominent and frequent genus during the fourth millennium, with the common hazel (*C. avellana*) stretching from Germany to the Carpathians, and four species occupying the Crimea and the Caucasus (for example, the large 'Turkish hazel'), but a singular scarcity or absence in the central, Ukrainian area.

10. THE NUT(TREE). Like the hazel, the first nut(tree) word is reflected in the three western stocks—for example, Latin *nux* and Middle Cornish *knyfan*—and goes back to a western Indo-European root, *knw-*. This *knw-* is geographically in complementary distribution with a second nut word, reflected in four relatively eastern stocks: Baltic, Slavic (Bulgarian *oréh*), Greek (Hesychius) *árua*, and Albanian *arrɛ:*; regarding the second set, Frisk (1954:157) was probably right: 'das nähere Verhältnis dieser Wörter zu einander bleibt noch aufzuklären'.

The pollen of the walnut (*Juglans regia*) has been found on the southern Volga (Frenzel 1960:391), and the tree is thought to be native to a broken strip running from the Caucasus to the Crimea—abutting on or overlapping with the southern part of the Proto-Indo-European homeland. The chestnut (*Castanea sativa*) was actually *the* dominant tree on the western slopes

of the Caucasus during the Boreal, dropped drastically (to under 5%) during the Atlantic, and was eventually spread across most of southern Europe by man.

With two botanical referents and two proto-terms (not counting the hazel), it seems certain that many of the Proto-Indo-Europeans had a concept of the nut and the nut tree. But the available evidence permits only speculative assignments of particular meanings to particular terms. One such hypothesis is that the *knw-* of the western tribes was used for the only nut *sensu stricto* that was present, forming a dyad with the likewise western *kos(V)lo-*, 'hazeltree'. Elsewhere, *ar-* (probably with distinguishing modifiers), was used for the three nuts and nut trees—the hazel, the walnut, and the chestnut, and possibly also for the fruit of the beech and the hornbeam.[11]

11. The Elms. Excellent correspondences in form and meaning between Latin *ulmus*, Middle Irish *lem*, and, among Germanic examples, Old Norse *elmr*, lead back to a western Indo-European feminine o-stem, *lmo-*, 'elm'. A second set includes Baltic and Slavic, and otherwise ranges from Kurdish *vi:z* and *bu:z*, to Albanian *vith*, *vidhi* (from **venza*; G. Meyer 1891:472), to Modern English 'wych elm'. The cognates in these five stocks descend from a pan-PIE *wyǵ(h)-*.

Ulmus, after a long period of inconspicuousness, increased rapidly toward the end of the Boreal, often at the expense of the hazel and closely followed by the linden. Climax forests of elm, often mixed with linden, developed over a large area from the Baltic to the Ukraine and the Caucasus (Straka 1960:313; Frenzel 1960:393). At least three species were present: the mountain, the common, and *U. laevis*. During the late Atlantic and the Sub-Boreal the genus declined drastically. This drop, while usually attributed to climatic change, was probably also due to Neolithic man, who stripped the elm for bark and polled it for foder, greatly reducing pollen output and the tree itself (as demonstrated in Troels-Smith's penetrating analysis).

The Germanic evidence for the elm unit, like that for the maple, suggests a prehistoric taxonomic relation. Specifically, the Modern English 'elm' versus 'wych' carries back to Proto-Germanic antecedents, and probably into a late and western stage of Proto-Indo-European, when *lmo-* and *wyǵ(h)-* symbolized perceived differences between two classes of *Ulmus*.

12. The Linden. The linden ranks between the hazel and the pine in the copiousness of its pollen, which has been found in deposits throughout European Russia and the Caucasus (Nejshtadt 1957:328); probably involved were three Russian species and, in any case, both the winter and the summer linden (which can be distinguished from each other palynologically). *Tilia* declined rapidly during the late Atlantic and early Sub-Boreal before the advance of its competitors, the spruce, the fir, and the beech, and because of the polling activities of man, presumably including the Proto-Indo-Europeans. Linden wood and the bark (bast) were probably important in the primitive technology of the Proto-Indo-Europeans, and the tree

and its groves of considerable significance in religious ritual. On botanical and ethnological grounds, one would expect a word.

Two potential sets of cognates may be rejected (e.g. Greek *pteléa*, borrowed into Latin and Armenian, where the forms mean 'elm'). A third set apparently is supported by obviously cognate Baltic and Slavic forms (e.g., Lithuanian *líepa*, Serbocroatian *lïpa*), and by Welsh *llwyf* (a conflation from **leimo*, 'elm,' and **leipro*, 'linden'), and possibly by the Greek *alíphalos*, 'oak bark' (Cuny 1868:199)—although Fraenkel (1962:366) flatly rejects the Greek and Welsh forms. Beyond the PIE form supported by Slavic, Baltic, and Celtic, the tree name cannot be recaptured, but the evidence suggests a derivational relation with a root for 'slippery, smeary, sticky' (*lyp-*), and an early taxonomic relation between the linden and the elm.

13. The Ash. PIE *os-* stands with *wyt-*, 'willow', and *dorw-*, perhaps 'oak', as one of the most widely attested tree names—eight stocks, including Baltic, Slavic, Germanic, and Albanian. Some dialects show k-extensions, as in the case of Proto-Greek *oskes* and Armenian *haçi* (Hübschmann 1897:465), whereas n-extensions are evidenced by others such as Latin *ornus* and Welsh *onn-en* (from **osna*; Pokorny 1959, 1:782). In most of the daughter languages the reflexes appear to denote the common ash (*Fraxinus excelsa*).

The genus is easily distinguished, but its species are not, and the pollen record is imperfect. Frost-sensitive *Fraxinus* (probably *excelsa*) probably did not enter much of eastern Europe until the Middle Atlantic (after the elm and linden), when it persisted as a minority member of mixed hardwood forests, before being considerably reduced during the Sub-Boreal by the polling activities of man, and the advance of the beech; ash saplings cannot grow in the deep shade of beech forests.

PIE *os-*, with modifiers, probably served to denote the common ash, plus some combination of the physically similar flowering ash (*F. ornus*), and the so-called 'mountain ash', and the *Fraxinus oxycarpa* of the Caucasus and the Cossack steppe.[12]

14. The Oak. *Quercus* entered shortly after the glaciers, but remained a minority genus for about five millennia. By the early Atlantic it abounded in the Cis-Caucasus, where it was probably already highly differentiated,[13] and extended far north into what is now the Cossack steppe. Two species, *Q. petraea* and the brown or English oak (*Q. robur*), reached percentages of 20 per cent to 30 per cent in the Ukraine and by the Middle Atlantic were flourishing three hundred miles north of the present line of the English oak. During the same centuries, the English oak, subsequently mixed with the winter oak (*Q. sessiliflora*), became frequent or dominant in western Europe at the expense of their two principal predecessors, the elm and the linden, and also of the hazel, alder, maple, ash, and pine; mixed oak communities and climax forests of oak became typical. *Quercus* was the focal tree in many areas during the great hardwood efflorescence of the Middle and Late Atlantic. During the sub-Boreal it partly surrendered to the beech and fir.[14]

The four PIE oak terms are:

1. *gʷelə-*, 'acorn', a generally accepted root often cited in discussions of the voiced palato-alveolar, and supported by Armenian, Albanian, Slavic, Italic (Latin *glans*), Baltic (Latvian *zīle*), and Greek *bálanos*; with the addition of Armenian, these are the same stocks as support the hornbeam.

2. *ayg-*, supported by Germanic (e.g. Old Norse *eik* 'oak, ship'), by Greek *krát-aigos*, 'hard-oak', and possibly words for weapons such as *aiganen*, 'spear', and by Latin *aesculus*, 'mountain or evergreen oak' (these same three stocks support the second maple word, *akVrno-*).

3. *perkʷw-*, supported by tentative forms from many stocks but not Tocharian, and Anatolian:

a. cognates are moot or otherwise questionable in Venedic, Illyrian (A. Meyer), Albanian (*Perëndija*), and Armenian *orot*, 'thunder'.

b. forms may come from a verbal root for 'strike': Slavic *Perun*, 'god of thunder, lightning', and Baltic (e.g. Lithuanian) *Perku:nas*, the god who 'incarnates the sacred oak' (Watkins 1964:45).

c. Germanic 'winter oak', as in Old High German *fereh-eih*, and many names of places and divinities such as Old Norse *Fjǫrgynn*, 'thunder god'.

d. Vedic *Parjányah*, personification of 'la pluie d'orage' and other ancient and modern Indic forms denoting oaks and sacred trees.

e. Latin *quercus*, 'oak' (with the initial consonant shifted by assimilation) matches phonologically point for point with the Celtic *Hercynia* (from *Perkúnia*:), which referred to the oak forests of the Middle German Highlands of Caesar's time;

4. *dorw-* has reflexes in all 12 stocks, including Anatolian (Hittite) *taru* and Tocharian AB *or*, both of which mean 'wood'. Many formal grades are attested (*drw-*, *dorw-*, *derw-*, etc.), and the fascinating range of meaning includes pine (in Germanic), larch, fir, spruce, tar, hard (in Latin), spear, spoon, bow, rainbow (in Modern Persian), tray, tree, and wood. A Germanic set meaning truth, belief, contract, and similar notions, was masterfully discussed by Osthoff (1901), who thought 'oak' to be the aboriginal denotation, and by Benveniste (1954:257–9), who prefers to envisage an abstract property such as 'hard' as the semantic starting point.[15]

The reflexes of *dorw-* denote 'oak' in only two stocks: (1) Celtic, where this is one of the usual meanings (e.g. Old Irish *daur*), and (2) Greek, where *drũs* occurs 17 times in Homer, usually as oak (e.g. *Iliad* 16:482), but sometimes as tree; Homeric *doru* meant 'wood.'[16]

Early Indo-European *dorw-* may have meant 'tree' or 'oak', or both—an ambiguity which was certainly present in the later stages of some dialects and which carried down to the Homeric Greeks and Caesar's Gauls. On the other hand, I would also advance the tentative hypothesis that the three words may have served to denote three distinct species:

ayg-, the live, stone, or possibly, the holly oak

perkʷw-, the winter oak (which diffused later)

dorw-, the hard and nearly ubiquitous English oak[17]

15. The Hornbeam. During the Boreal, *Carpinus* was absent from the Ukraine, but with the subsequent Atlantic this moisture-loving, shade-

eywo, meaning 'yew' or 'tree with red berries', may be cognate with the Hittite *eḫan*, an evergreen tree with yew-like legal and religious functions (Pritchard 1955:191,348; Ehelolf 1938:col. 4, lines 17–21; J. Friedrich 1952). However, the total phonological and semantic case for *eywo* is weak.

The second yew word is solidly attested by Latin *taxus* and the *ti:so-* of Proto-Slavic (where /s/ would be the normal reflex of PIE /*ḱs*/). The Medieval Iranian word for 'bow' (*taxš* in Firdausi) corresponds well formally and fits the already-mentioned pattern whereby a tree name also refers to or totally shifts to a typical product (compare elm:bow in Old Norse, and ash:spear in Homeric Greek). The same arguments hold for the *tókson*, 'bow', of Greek, where a second, borrowed word served to denote the yew tree (Chantraine 1933:378).[20]

Conclusions

My conclusions fall into three parts: first, a threefold classification of the tree names, with particular attention to their attestation in the daughter stocks; second, a cursory review of the botanical evidence; third, a final summary of the 18 arboreal units.

The 27 tree names can be classified into three groups. The first of these comprises names that are weak on linguistic or semantic grounds or both. The second group consists of 12 tree names that are strong in terms of the same two criteria. The third group—which is of a quite different order—includes all of the second group, and some of the first, and depends in part on evidence that is relatively precarious. Some names in this third group may hark back to early or pre-Proto-Indo-European.

Let us now begin with the first group, that of the 16 weak cases. Fifteen other tree names are weak in one or more of three ways:

1. The cognates are linguistically problematical (the juniper-cedar, apple, hornbeam, linden, and cherry).

2. The attesting stocks are only three in number (e.g. the juniper-cedar, the aspen-poplar, and the second maple word, *akVrno-*); a particularly interesting subset here is the three western stocks (Celtic, Italic, and Germanic), which provide evidence for the hazel (barring improbable cognates in Baltic and Slavic), and for one of the words for the nut (*knw-*) and the elm (*lmo-*).

3. The two words for the nut (tree), *knw-* and *ar-*, although in complementary distribution geographically, cannot be assigned definitely to any of the botanically attested nuts (the hazel, the walnut, and the chestnut).

The stocks supporting the weak tree names fall rather dramatically into two groups. The first includes, in order of strength, Italic and Slavic (9-2 and 8-2), and Greek and Germanic (both 8); given the problems of evaluation, these top four are essentially on a par. Baltic is fifth (7-3), and Celtic sixth (6-1). The second group—coming after a considerable and significant break—comprises Albanian (1-3), Armenian and Anatolian (both 2), Indic (-3), and Iranian and Tocharian (both -1).[22]

Among certain of the stocks which strongly support the weak tree names,

	Gr	Celt	It	Gk	Slav	Balt	Alb	Arm	Anat	Ind	Ir	To
1. el-n/w-				+	+	?		+				
2. asp-	+				+	+				?	?	
3. wrb-			+		+	+				?		
4. abVl-	+	+	+		+	+						
ma:lo-			+	+			?		+			?
5. akVrno-	+		+	+						?		
6. kosVlo-	+	+	+		?	?						
7. knw-	+	+	+									
8. ar-				+	+	+	+					
9. lmo-	+	+	+									
10. lyp-		+			+	+						
11. ayg-	+		+	+								
12. gro:b(h)-			?	+	+	+	?					
13. Ker-n-			+	+	?	?	?					
14. eywo-	+	+	?	+	+	+		+	+			

CHART I: THE WEAK SET

there at least two sets the members of which are associated with each other in a way that is clearly not random. First, Baltic and Slavic each support the same ten tree names in nearly identical degrees of validity; on the other hand, the association between these two stocks and the likewise 'central' Germanic, is surprisingly low (only on the apple, aspen-poplar, and yew, plus two involving egregiously poor cognates). Second, the familiar western trio of Celtic, Italic, and Germanic support the same five tree names out of a total of 12 supported in any way by either; this association is more meaningful because of the greater number of stocks involved. On the other hand, the total of six shared by Latin and Greek out of the total of 13 supported in any way by either, is suprisingly low. Also of interest is the complementarity between the first, Balto-Slavic set, and the western set, the main exceptions being the yew name and the first apple name (see Chart I).

As for the number of stocks supporting each tree name, the range is from two (*lyp-*, *ker-n-*), to four (*ar-*), and five (*abVl-*), to seven in the case of the semantically problematical reflexes of *eywo-*. Thus, the great majority are supported by three stocks (discounting very weak cognates). Other conclusions regarding the weak cases are taken up below.

A second group—that of the strongly attested tree names—includes ten arboreal units (twelve names) that are reflected in numerous stocks (four to nine), and in every case at least two of the supporting stocks are noncontiguous: birch (*bherHǵo-*), pine (*pV-*; *pyt/ḱ-*), willow (*wyt-*, *sVlyk-*), alder (*Vlyso-*), maple (*klen-*), elm (*wy̆ǵ(h)-*), ash (*os-*), acorn (*gʷelə-*), beech (*bha:ǵo-*), and the yew (*tVḱso-*). Within the strong group I would also include the second oak name (*perkʷw-*), although many of the cognates in the nine stocks have been challenged or rejected. The second yew name (*eywo-*) has been included on the chart, although I do not regard it as equal in strength to the others.

What conclusions may be drawn from the twelve strong cases? All but

	Slavic (Russian)	Germanic	Italic (Latin)	Greek	Baltic (Lithuanian)	Celtic	Others, Total
1. bherHǵo-	berëza	Birke	fraxinus		bèržas		Indic, Iran. (6)
2. pVt/ḱ-	pekUlU	Fichte	pi:nus picea	pítus peúke:	pušìs	Mid. Ir. ochtach	Indic, Alb. (8)
3. sVlyk-		OHG salaha	salix	helikó:n		Old Ir. sail	Anatol. (?) (4–5)
4. wyt-	vetlá	Weide	vi:tis	oisos wi:téa:	vytìs	Irish fe:ith	Ind., Iran. Arm., (9)
5. Vlyso-	ol'khá	Erle	alnus	Mac.álidza? H.Olidzo:n	elksnis	Proto-Cel. alisa	(6)
6. klen-	klën	Proto-Ger. hluniz		klinó- (?)	klẽvas	kelyn	(4–5)
7. wy̆ǵ(h)-	vjaz	wi:ke			Latvian vîksna		Iranian, Alb., (5)
8. os-	jásen'	Proto-Ger. askiz	ornus	H. oksúe: Proto-Gk. *osk[e]s	úosis	Welsh onnen	Alb., Arm., (8)
9. gʷelə-	zhëlud'		glans	bálanos	gìli	Old Ir. sail	Arm., Alb., (7)
10. bha:ǵo-	bu/ozU	Old Norse bo:k	fa:gus	pha:gós		Gaul. Bace:nis(?)	Iran. (?), (4–6)
11. tVḱso-	tis		taxus	H.tókson			Iran., (4)
12. perkʷw-	Old Russ. PerunU	OHG fereh-eih	quercus		Perku:nas	Hercynia	5 others, (10)
13. eywo-	íva	Eibe	u:va	Proto-Gk. oywa:	ievà	Welsh yw(en)	Arm., Anat., (8)

CHART II: THE STRONG SET
(plus *eywo-*)

one (*g*^{w}*elə-*) are supported by Germanic, and all but one (*sVlyk-*) by Slavic. All but two (*wĭǵ(h)-* and *klen-*) are supported by Italic, and all but three (*bha:ǵo-*, *tVḱso-*, and *sVlyk-*) by Baltic. Greek supports all but four (*wĭǵ(h)-*, *bherHǵo-*, *perk*^{w}*w-*, and probably *Vlyso-*), and Celtic all but five (the same list as Greek, but adding *g*^{w}*elə-*, and substituting *tVḱso-* for *perk*^{w}*w-*). Otherwise, Albanian, Armenian, and Indic are of about equal strength, each supporting strongly four out of the twelve tree names, and each providing one or two additional weak cognates. Not one of the strong tree names is supported by Tocharian or Anatolian (barring two unlikely cognates of *sVlyk-*). As indicated on chart II, the total number of stocks supporting a tree name may be four (e.g., *tVḱso-*), five (*wĭǵ(h)-*), six (*bherHǵo-*, *Vlyso-*), seven (*g*^{w}*elə-*), eight (*pV-*, *os-*), or nine to ten (*perk*^{w}*w-*), plus several transitional cases.

Let us now sum up regarding both the weak and the strong cases. In both sets we find a singularly high degree of phonological and semantic correspondence between the five stocks which in some important sense constitute our main platform for reconstructing the PIE arboreal nomenclature: Baltic, Italic, Germanic, Slavic, and (a bit lower) Celtic. Indeed, cognates for almost all 12 strong names can be found in the limited sample of four single languages: Classical Latin, and Modern Lithuanian, Russian, and English, and of the 18 PIE arboreal units, the terms for 16 and 14, respectively, have carried down into the last two of these idioms.[23]

Both the weak and the strong cases are less impressively supported by five further stocks. Greek is strong in both sets, with seven good and two poor cognates for the weak names, and two thirds support for the strong names. But almost half of the solid reflexes in this stock show a shift in denotation—eight out of the total of 17. Albanian, Armenian, and Indic, although weak in both sets of names, each provide a total of four or five relatively good cognates and one to three weak ones. Iranian, though of medium strength in the strong set (five out of 12), falls away sharply in the weak set to one weak cognate. With these qualifications, the five stocks in question, on the southern and eastern marches, often help greatly in determining the Indo-Europeanness of a tree name—particularly Indic, with its interesting reflexes for the birch and, some would argue, the maple and aspen-poplar.

Last, for both the strong and weak sets of tree names, Tocharian yields only one moot cognate, and Anatolian provides only four, none of which have been generally accepted.

A third and final set consists of tree names that because of their form (e.g. providing evidence for the laryngeals), or the number, spread, and geographical discontinuity of the attesting stocks (e.g. reaching to Indic or Hittite), or similar considerations, are to be assigned to early or pre-Proto-Indo-European (the first part of the fourth millennium).[24] This third, archaic set includes the following:

1). All of the second group, particularly the birch (*bherHǵo-*), the pine (*pV-*), ash (*os-*), and one of the oak terms (*perk*^{w}*w-*).

2. An ancient word for the maple, *kL-n-*, reflected in PIE *klen-* and *akVrno-*.

3. A second word for the yew, *eywo-*, appears to have an Anatolian reflex in the Hittite *e̯ian*, a ritually significant evergreen tree, and an impressive array of formally corresponding terms (four of which more or less support a 'yew' meaning).

4. *Dorw-* constitutes a special case, partly because of the unique number of supporting stocks (all 12), and partly because of the extraordinary variety in the denotative reference of the various reflexes. *Dorw-* probably meant oak, or tree, or both, with the exact referent determined by the speech situation. On the other hand, the denotation of *dorw-* may have been a single species—the English oak (*Quercus robur*).

Let us turn from the weak, the strong, and the pre-PIE tree names to a brief summary of the botanical evidence. Botanically, the linden and the hornbeam are well established for the Atlantic. The pollen evidence for the juniper-cedar, the aspen-poplar, and the cornel cherry is adequate to demonstrate their presence in some regions. The walnut and the chestnut were limited to the southern and eastern parts of the homeland area. Recent, historical distributions and other physical evidence make me think that both the wild apple and the bird cherry were in the PIE environment.

Some of the twelve trees referred to by names in the strong set above, such as the birch, pine, willow, and alder, had been present since the glaciers and were practically ubiquitous during the Atlantic and sub-Boreal. In fact, palynology establishes the presence of all but one, the yew, which does not leave a pollen record; yet the yew's present range and ecology suggest that it, too, was present on the western and southeastern peripheries. My conclusion is that the twelve trees in the strong set were well known to all of the PIE speakers during the last millennium or so of their unity as mutually intelligible dialects.

The conjunctive approach has led to improving the definitions of the botanical referents of the PIE tree names from some relatively vague words in modern European dialects—often reflecting guesses by the urban Indo Europeanist in his library—to relatively precise glosses such as would characterize the perceptions and nomenclature of a primitive and partly forest-dwelling people. The tree units are given below, using English language terms as ready labels, and then giving the reconstructed PIE symbol, and then the species and genera in botanist's Latin. As in the second section of this article, the tree units are listed roughly in the order of their geological succession from the Boreal through the Atlantic to the Sub-Boreal:

1. the 'birch', PIE *bherH́go-*, denoting *Betulus* (mainly *B. alba*);

2. the 'Scotch pine', pre-PIE *pV-*, PIE *pytw-*, *pwḱ-*, denoting *Pinus sylvestris* (and perhaps other, additional conifers on the peripheries);

3. the 'juniper-cedar', late PIE *el-n-/el-w-*, denoting *Juniperus* and *Cedrus*

4. the 'aspen-poplar', regional PIE *asp-*, denoting *Populus* (mainly *tremula*);

5. the 'willows', PIE *wyt-*, *sVlyk-* (and perhaps *wrb-*), denoting *Salix* (and possibly involving a taxonomic dichotomy between the osiers and the tree willows);

6. the 'apple', northern *abVl-* and southern *ma:lo-* denoting *Malum* (probably some variety of the wild apple);

7. the 'maples', PIE *klen-* and late PIE *akVrno-*, denoting species of *Acer* (and probably symbolizing a perceived dichotomy between two physical classes); both terms may go back to an early PIE *kL-n-/kVL-n-*;

8. the 'alders', PIE *Vlyso-*, denoting *Alnus* (and probably covering four regionally limited species: the black, gray, bearded, and mountain alders)

9. the 'hazel', western PIE *kosVlo-*, denoting *Corylus* (mainly *C. avellana*, but probably including other species such as *colurna*);

10. the 'nut(tree)', PIE *ar-* and western PIE *knw-*, the latter probably associated with the likewise western *kosVlo-* and used for the hazelnut, whereas the former was used in the east for various kinds of nut(tree), including the walnut (*Juglans regia*) and the chestnut (*Castanea sativa*);

11. the 'elms', PIE *wy̆ǵ(h)-* and western PIE *lmo-*, denoting *Ulmus* (the contrasting terms in the western stocks probably symbolized a perceived difference between two classes of elm);

12. the 'linden', the genus *Tilia* was ubiquitous physically and probably important technologically, but the PIE term is only supported by Balto-Slavic *lyp-*, and the tantalizing Welsh *llwyf*;

13. the 'common ash', PIE *os-*, denoting *Fraxinus excelsa* (plus probably some combination of the mountain, flowering, and oxycarpal species)

14. the 'oaks', PIE *ayg-*, *perkʷw-*, and *dorw-*, denoting species of *Quercus*, and possibly distributed among the three main species: *petraea*, *sessiliflora*, and *robur* (on the other hand, the latter two terms may have meant 'tree');

15. the 'common hornbeam', PIE *gro:b(h)-* or *gra:b(h)-*, denoting *Carpinus betulus*;

16. the 'beech', PIE *bha:ǵo-*, denoting *Fagus*, probably both *sylvatica* and *orientalis*;

17. the 'cherries', problematical early PIE *Ker-n-*, denoting the bird cherry (*Prunus padus*), the cornel cherry (*Cornus mas*), and possibly other species of *Prunus*;

18. the 'English yew', PIE *tVḱso-* and early PIE *eywo-*, denoting *Taxus baccata.*

Notes

[1] This paper, an outgrowth of my presentation at Philadelphia, is also an abstract of the results contained in a forthcoming monograph (now about 250 pages), which gives more philological and paleobotanical detail, and focuses on the problem of reconstructing a taxonomy. Numerous scholars have contributed to this research, and all are acknowledged in the long version. For helpful comments regarding or affecting the present abstract, I am particularly indebted to Karl Butzer, Burkardt Frenzel and Homer Thomas (botany), Maurits Van Loon and William Wyatt, Jr (Greek and Anatolian), Zbigniev Gołab and Ben Howe (Slavic), Winfred Lehmann and George Metcalf (Germanic), Alfred Senn (Baltic), Hans Güterbock (Hittite), Manfred Mayrhofer (Indic), Nancy Spencer (yew), J. Kurłowicz, Eric Hamp, James Redfield, Robbins Burling (introduction), and Margaret Hardin Friedrich, who

thoroughly criticized both versions. I owe a general intellectual debt to Henry Hoenigswald, and to the archaeologists, Lewis Binford and Stuart Struever.

My analysis was enormously helped by the lexicography of Bartholomae, Fraenkel, Frisk, Hübschmann, Kluge, Mayrhofer, Meillet, Pokorny, and Vasmer.

My summary statements on the arboreal prehistory of central and eastern Europe are derived mainly from four recent and authoritative sources: Firbas, Frenzel (1960), Nejshtadt, and Walter. Some western scholars are skeptical about this east European evidence because we still lack both carbon-14 dates from the USSR (with one exception), and also interlocking pollen diagrams such as characterize the studies in Germany, the Netherlands, Scandinavia, and the British Isles. However, I suspect that if and when we get comparable data from the USSR, it will not greatly alter the picture of what trees were present in the PIE homeland during the Atlantic.

In the following pages Greek theta is symbolized by (th), the Slavic low high vowels (ĭ, ŭ) by (I, U), and vowel length by a colon; (kh) stands for the (χ) of Greek and the (x) of Slavic; (V), (L), and (K) stand for a vocalic nucleus, a liquid, and a velar stop the more exact shape of which cannot be determined. I have only starred the forms where the context does not make it clear that they are reconstructions and do not occur in texts. H stands for a laryngeal (e.g., *bherHǵo-*). Hyphenated stocks are used in some cases (Balto-Slavic, Italo-Celtic) as a shorthand indication that an ancestral form was shared; hyphens do not indicate a specific commitment to theories of tribal or national unity. Following botanical practice, the Latin names of genera are capitalized, and abbreviated to the first letter when followed by the species name, which is always in lower case.

[2] Since we lack PIE texts and since methods of syntactic reconstruction are weak at best, syntax is nearly irrelevant to this study. As for the morphology, there are reasons for thinking that the PIE tree symbols were almost all unanalyzable roots; my painstaking review of the evidence has uncovered only one PIE tree category (the willow) with names that may well be derived from a verbal root. In two non-contiguous modern languages, English and Russian, practically all major tree names consist of or are built up from such roots (birch, pine, aspen, etc.). In Tarascan—the only non-Indo-European language that I have thoroughly studied—almost all native trees are symbolized by separate roots. The basicness of names for trees in primitive language and culture systems may be a fairly general phenomenon. This renders Proto-Indo-European derivational morphology *relatively* less relevant than the phonological analysis of the root forms; for a superb discussion of the role of morphology in PIE etymology, see Szemerényi (1962).

[3] Clearly, a prerequisite of an adequate analysis is that it take account of all kinds of meaning—metaphorical extensions, metonymic shifts, and the like—since some of the most valuable albeit delicate inferences must rest on this basis. However, for reasons of brevity, this abstract has been curtailed to botanical denotation and its relation to the botanical evidence.

[4] Admittedly, this suggests a confusion, since some scholars would use semantic—I think inappropriately—for the overall study of meaning and culture, and, in the present case, for the results of integrating all three analytical systems.

[5] For negative examples, see Isachenko's article on PIE kinship (1953), based on nineteenth century theory, and some of Dumézil's work on mythology and religion, based on Durkheimian theory only somewhat less out of date.

[6] The shift of the PIE birch term to ash in Latin *fraxinus* was congruous with the shift in the same stock of the ash term (*os-*) to mountain ash—or with the retention of this latter meaning from a very early period.

[7] For a trenchant rejection of this term, see Benveniste (1955:30–1).

[8] Middle and late PIE speakers in the Caucasus and Carpathians presumably used a more finely discriminating nomenclature to cover the relevant species of fir, spruce (*P. excelsa*), and the "crooked" and mountain pines. As a comparative note, the Tarascans of the high sierra regularly recognize and label two more species of pine (*kani-śU*, *uRú-śU*) than do many of the Indians in the temperate zone.

[9] One set of possible cognates in four stocks denoting 'elder', deserves a footnote. Latin *sambucus* may belong with the Slavic *boz/baz/buz* (which are often treated as cognates of the beech set), the latter going back to *buzg-*, *busk-*, and *sbuk-* (Machek 1950). To this evidence may be added Gallic *skobie*: and Dacian *seba*.

[10] Armenian *op'i* 'white poplar', mentioned by Eilers and Mayrhofer (1962), needs further study by a stock specialist.

[11] A special problem is that one often cannot know what nut the lexicographer meant by *Nuss*. Greek *kástanos* (the source of *Castanea*, etc.), was probably a loan (Frisk 1954:799).

[12] Since the remarkably cold-resistant and generally hardy mountain ash (*Sorbus aucuparia*) had probably been present since the glaciers, and is more widely attested in Atlantic pollen sites, one *could* argue that this was the original denotation—in which case, Classical Latin *ornus* 'mountain ash', alone would have preserved the original denotation. On the other hand, Latin *sorbus* 'mountain ash, service tree', and Old High German *sperboum* 'mountain ash', and Slavic *ber-/bre-*, '*Sorbus torminalis*', sufficiently resemble each other in form to have motivated Machek (1950:156) to argue for the existence of a separate PIE word for the mountain ash.

[13] Today in the Caucasus there are six main species and—according to one Soviet authority—a grand total of 17.

[14] The spruce expanded during the same time in the same area (except the Ukraine), and also witnessed a withdrawal during the sub-Boreal.

[15] This is a Germanic phenomenon, and probably derives from a Proto-Germanic strong verb.

[16] The oak terms in both these stocks have religious meanings, as do several reflexes of *perk^{w}w-* (in four different stocks: Baltic, Slavic, Germanic, and Indic)—all of which points toward a symbolic set of priests, high gods, thunder and lightning, and the oak.

[17] In Tarascan, for example, three kinds of Mexican oak are denoted by three distinct and, to my knowledge, unanalyzable roots: *tukú-śU*, *uRí-kua*, and *śUnchá-mu*.

[18] *Carpinus betulus* is related to the American hornbeam or 'blue beech' but should not be confused with the skewer- or spindle-wood tree; both are sometimes called 'ironwood'.

[19] Thus, the exact form and meaning of the beech name in Proto-Indo-European remains more doubtful than for many other trees; see Eilers and Mayrhofer (1962) for an excellent discussion. The beech is limited geographically by a subtle combination of surface dryness, late frosts, and the length of winter cold and summer drought; consequently, its distribution varies considerably from one geological period to another. The attempts to use the beech line in homeland arguments have not taken account of these variables (Bartholomae 1918; Thieme 1953).

[20] The bow, an ancient component in Greek culture (see, for example, Iliad 9. 559), was perfectly well known to the Mycenaean Greeks (Ventris and Chadwick 1959: 356, 361, 404), and to the Homeric Greeks (25 textual occurrences), and to the post-Homeric Greeks, whose opponents and Scythian mercenaries wielded it with deadly effect. Benveniste, however, argues (1937:37–41, reiterated in 1966) that Greek *tókson* was a Scythian loan, largely on the grounds that the Greeks themselves did not know how to use the bow.

[21] Other familiar European trees are the acacia, laurel, myrtle, and plane tree, but these were probably absent from the PIE environment. The larch (*Larix*) was certainly present in the pre-Boreal and Boreal, but had probably been driven out well before the Atlantic; except for certain highland areas such as the high Carpathians, this cold-tolerant geological pioneer was probably absent from even the peripheries of the PIE homeland; a PIE term cannot be reconstructed.

[22] The first parenthetical number refers to the unambiguous cognates, the second to the more moot or unlikely ones.

[23] The four stocks provide the core evidence in a way that somewhat resembles the function of Vedic, Avestan, and Homeric Greek within the verbal system as pre-

sented by Meillet (1937:V), with the important difference that the primacy of the four stocks within the arboreal evidence has emerged as a result of a relatively simple induction.

[24] Particularly striking cases of good cognates in discontinuous stocks include *pV-* (Celtic and Baltic), *wyǵ(h)-* (Germanic and Iranian), *eywo-* (Celtic, Germanic, and Anatolian), and *gʷelə-* (Baltic, Albanian, and Armenian), and of course *dorw-*, *perkʷw-*, *os-*, and *wyt-*. The relative paucity of evidence from Albanian, Armenian, Anatolian, Tocharian, and Hittite presumably reflects various combinations of factors such as substratum influence (Anatolian), sociopolitical subordination (Albanian), the nature of the texts (Tocharian), or migration to a radically different arboreal environment (Indic). Of these five stocks, all but Armenian retained reflexes of *dorw-* and the apple word, and, as noted, Albanian shares several shifts and losses with Greek; otherwise, the arboreal nomenclature in the five peripheral stocks appears to have followed independent paths. These facts, like the degrees of supporting evidence, and the sets of nonrandom association discussed above, bear significantly on questions of intertribal contact and prehistoric tribal migrations.

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Inscriptional Evidence of Early North Germanic Legal Terminology

Otto Springer

I

Quite a number of years ago I presented to a meeting of the Linguistic Society of America a paper in which I tried to analyze the inscription of the famous Eggjum Stone on the basis of the phoneme distribution in early, that is, preliterary North Germanic. As many members of the Conference will know, this runic inscription with its 173 graphs and approximately 50 words, written largely in the older runic futhark, is one of the longest runic inscriptions that we have; it is also one of the most recent to be unearthed. As late as 1917 it was stumbled upon by a Norwegian farmer who was ploughing his fields near Eggjum not far from the Sogn Fjord in Southwestern Norway.[1]

I shall concentrate in this paper on ONE particular word contained in this inscription which, if my reading of FOUR BADLY DAMAGED RUNES and my interpretation of the restored word are correct, will present some unexpected linguistic evidence of early North Germanic legal terminology and, in this sense, of a certain aspect of the social structure of preliterary medieval Scandinavia. To that extent, and to that extent only, I venture to contribute the following remarks to the general theme of this Conference.

However, for the benefit of the one word in question I must try to introduce you, as briefly as I can, to the gist of the entire inscription. Needless to say, if space permitted, I would much rather follow what, methodologically, would seem to be the only appropriate and safe procedure, that of starting out with the continuum of the inscriptional text as reproduced in Figs. 1 and 2.

The second step then would be a systematic attempt to dissect this continuum into lexical units and to do so on the basis of purely FORMAL criteria without regard for any probable or preconceived notions,—that is, as much as possible, without any reference to meaning at all. This is what I attempted to do in a previous paper and what I therefore will not repeat or elaborate on in this one (see Fig. 3).

For a runic inscription of the late seventh or early eighth century A.D., such as Eggjum, we have every reason to assume that there still prevailed at the time the fairly regular practice of NOT representing long or geminated

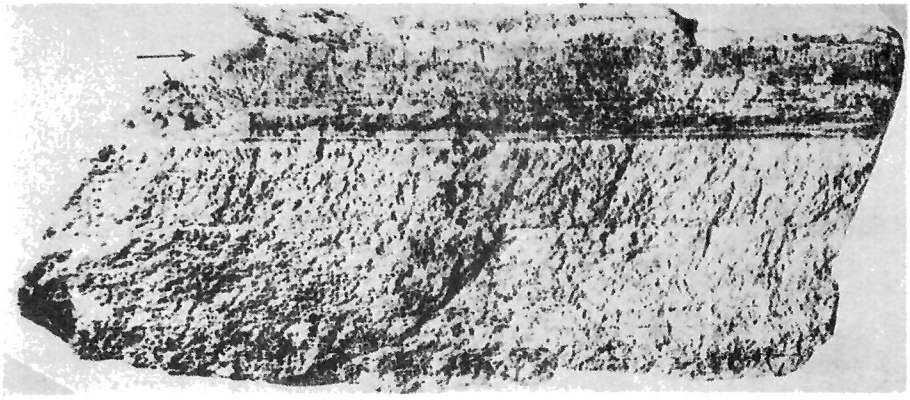

FIG. 1. The stone slab of Eggjum. The six illustrations in this paper were first used in connection with my paper "Zur Eggjum-Inschrift," published in *Zeitschrift für deutsche Philologie*, 85 (1966) 22–48. They are reproduced here with the kind permission of Erich Schmidt Verlag-Berlin.

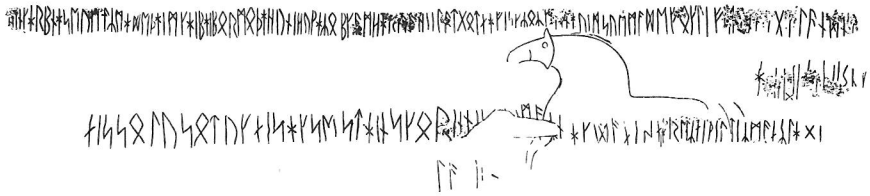

FIG. 2. The inscription.

consonants by two distinct signs UNLESS the two like consonants were separated by a word boundary. Consequently, wherever our inscription has two like consonants in succession, and this happens twice, we assume one to be final in the first word, the other initial in the second.

Furthermore, to proceed from graphemics to matters of phoneme distribution: for a runic inscription of the age of Eggjum we can assume the almost exclusive word-final position of Early Norse *R* (from Proto-Germanic voiced *z*)—the few exceptions are checked off easily enough. This

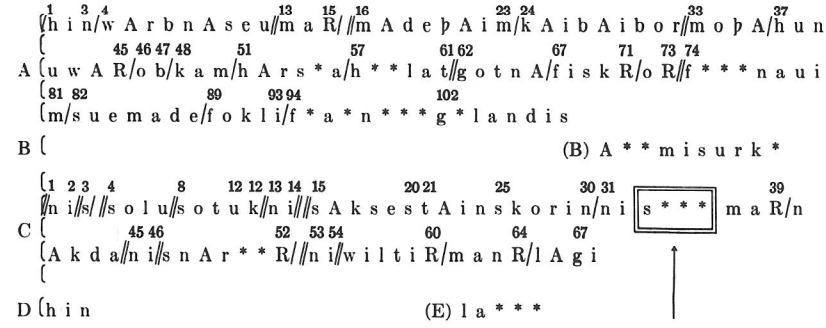

FIG. 3. The inscription transliterated.

(C)

ni s solu sotuk ni sAksestAinskorin ni s*maR nAkda ni snAr**R ni wiltiR manR lAgi**

(A)

hin wArbnAseu maR mAdeþAim kAibAibor moþA huni huwAR ob kam hArs*a hlat gotnA**

fiskR oR f*nAuim suemade fokli f*A*n***g*landis-**

(B)

Amisurk***

FIG. 4. Transliterated text with tentative word-boundaries (lines C-A-B).

criterion yields eight more word boundaries. Others can be inferred from the restriction of *f* (the voiceless bilabial spirant) and of *h* (whatever its phonetic characteristics) to word-initial position—again with very few easily controlled exceptions—, as well as from the isolation of a frequently occurring morpheme *ni*.

With the help of these and similar formal criteria we are able to make 34 cuts, that is, to establish 34 or two-thirds of the total number of word boundaries in an inscription which turns out to consist of hardly more than 50 words (see Fig. 4). The few and relatively short continua left can be resolved rather easily.

(1) *Ni 's s̲olu s̲ot*	*uk ni s̲Akse stAin skorin,*
"It has not been struck by the sun	nor has the stone been carved with (iron) sword.
(2) *ni s̲ . . . maR nAkda*	*ni s̲nAr(þi)R ni wiltiR manR lAgi.*
"neither a . . . man (shall lay it) bare	nor distressed nor stray men (shall do so)!
(3) *Hin wÀrb nAseu m̲aR*	*m̲Ade þAim kAibA i bormoþA huni.*
"The man sprinkled it with the 'corpse-sea' (i.e. 'blood')	smeared with it the oarlocks in the 'bore-tired' boat (jettisoned by drilling)
(4) *H̲uwAR ob kam h̲Ars(i) a*	*h̲(i a) lat gotnA?*
"Who has come hither on it	here unto the land of 'Goths' (i.e. men)?
(5) *F̲iskR oR f̲(lAi)nAu̲im suemade,*	*f̲okl i f̲(r)A(k)n(A il) g(A)landi.*
"The fish swimming out of the stream-of-spears (i.e. 'blood'),	The bird shrieking in the hail-of-lances (i.e. 'battle').

"(= 'sword' = ONorse ?) + (= 'spear' = ONorse ?)

(? + ?)

(6) *S-A (lu) misurk(i).*
"he died by a misdeed".

FIG. 5. Restored text with English translation (lines C-A-B).

Without going into any further detail and without noting the innumerable moot points in regard to transliterating, translating, and interpreting, an approximate modern English rendering of the inscription is given in Fig. 5.

And the meaning of the whole? A burial at night with a stone slab not touched by an iron implement, with a warning addressed to the most likely intruders not to disturb the grave. Then a somewhat obscure reference to a blood ritual, followed by the rhetorical question, as it were, concerning the identity of the dead. And the answer, not given in straightforward manner, but, probably for apotropaic reasons, couched in the form of a name riddle, a linguistic substitution, which under the technical term of *ofljóst* was to become a favorite pastime with the later skalds. The term itself is a paradox, meaning, as it does, 'all-too-lucid'!

In the case of a compound name, the Old Norse *ofljóst* works on the principle of substituting for each member of the compound a synonymous expression or a metaphorical phrase. If you permit me to digress for a moment and to illustrate this with a contemporary example—and if my friend Hoenigswald authorizes me to use his name and, in addition, condones the (to me highly improbable) identification of the first element *Hoenig-* with the German word for 'honey', then, to manufacture an *ofljóst* for '*Hoenigswald*', the first step would be to replace *Hoenig-* by something like 'bait of the bear' and *-wald* by, let us say, the 'home of the wolf'. However, with all this we have merely arrived at a 'one-story' *ofljóst*: in order to match the compounded trickery of our inscription, we now have to substitute something like 'the beast of the burrow' for 'bear', and, let

us say, 'the hound of hell' for 'wolf', so that we end up with a handsomely alliterating gem like this (an *oflj́ost* is supposed to alliterate!): 'the bait of the beast of the burrow' plus 'the home of the hound of hell'. . . . I dare any linguist to decode an *oflj́ost* of this kind and come up with the name *Hoenigswald*!

No doubt, the dead Norseman who once lay buried under the slab of Eggjum will forever remain nameless—in spite of half a dozen futile attempts to crack the code. We had better resign right now and concede that the runemaster's ancient linguistic spell has managed utterly to defy and to triumph over all modern linguistic analysis!

Indeed, it is a high degree of linguistic sophistication that emerges from this runic inscription discovered along the northern fringe of the Germanic world and recorded at a time before the first utterances in a Germanic vernacular found their way onto parchment in Central Europe and in the British Isles.

II

I repeat: a goodly portion of the Eggjum inscription remains controversial or obscure, especially in its second half. However, also in its somewhat more factual first part we are left with a number of tantalizing questions. One of the most troublesome is due to the badly damaged sequence of four runic signs, which in Fig. 3 has been indicated by an arrow and in Fig. 5 has remained blank in the first half of line (2): *ni s . . . maR nAkda.*

To date, as many as three debatable restorations of this passage have been attempted and, accordingly, three different interpretations of the whole line (see Fig. 6).

Each of these restorations must be rejected. Magnus Olsen's emendation of *sAti* 'he may set' (Fig. 6c) is paleographically impossible because of the shape of the *t*-rune; it would place the alliteration on a verbal form of very minor significance and destroy the obvious parallelism with the two adjectives *snArþiR* and *wiltiR* in the second half of the line; moreover, *sAti* just makes no sense.

Lis Jacobsen's restoration *sAid-*, which would yield *sAidmanR* 'sorcerer' (Fig. 6d), is even more impossible paleographically because the shape of the last letter does not fit into the picture at all. Moreover, why should the warning not to move the slab be addressed to 'sorcerers'? And what would be the relation of these 'sorcerers' to the *snArþiR* and *wiltiR* men of the second half of the line? There is no evidence for Dr. Jacobsen's contention that these epithets refer to bewitched people.

The third reading, suggested by Dr. Heiermeier (shown in Fig. 6e), namely *stigAmanR* 'highwayman', is the least probable of all. Paleographically it fits less than either Olsen's or Jacobsen's emendations because it has to squeeze in a fifth and rather fat rune sign for *g*, and it again operates with the misfit for *t*. Furthermore, *stígamaðr* seems to be a word of fairly recent vintage and, what is worse, its initial cluster *st-* alliterates only with

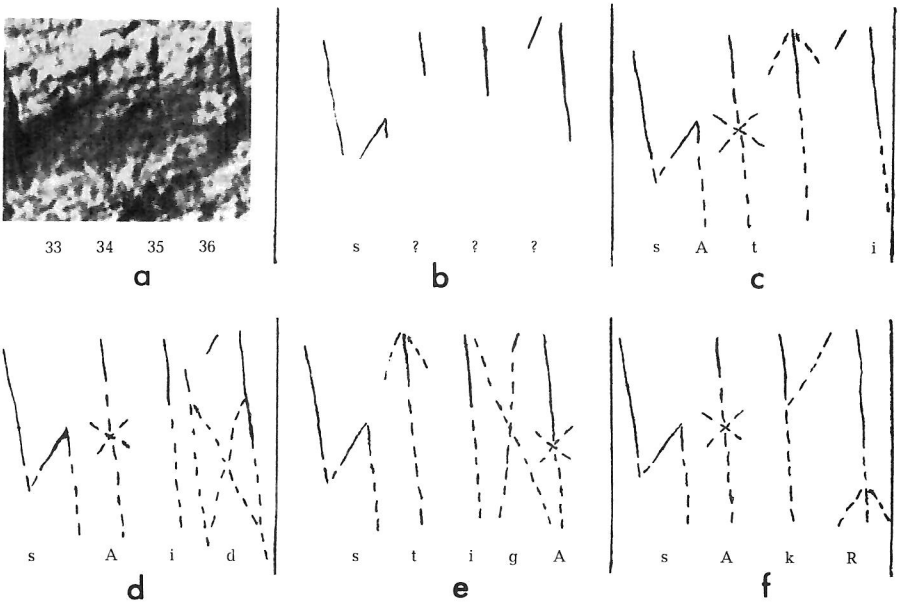

FIG. 6. Attempts to restore runes 33–36. a. Photograph of runes 33–36. b. Unrestored. c. Restored by M. Olsen. d. Restored by L. Jacobsen. e. Restored by A. Heiermeier. f. New proposal.

st-, not with *sn-* as in the following *snArþiR*, according to the rules of the game. The only good thing that can be said for Heiermeier's restoration is her endeavor to take into account the paleographical trace between the last two shafts, also her search for a realistic designation of people who might tamper with the grave and its covering slab, parallel to *snArþiR* and *wiltiR* men, which no doubt means 'distressed' and 'stray' people with no magic connotation whatever.

We have no choice but to start afresh and without any preconceived notion from what is actually left of the runes in question:

There is no doubt that the FIRST rune can only be restored to an *s*. Also of the vertical shaft of the FOURTH letter enough can be recognized to exclude every other runic character but *i* or *R*.

Of the SECOND rune the upper one-fourth or one-third of a vertical staff can be made out, restricting us to a narrow choice of six runic signs, namely *i*, *n*, *A*, *R*, *þ*, *s*, of which the last three cannot follow the initial *s*.

Of the THIRD rune as much as the upper third or even half is visible, so that we are again reduced to a choice from the same six characters as in the case of the second rune, unless—and this is important—we accept the (already mentioned) small but rather clear trace of a slanting stroke at its upper right and allow for one additional possibility, namely *k*.

After experimenting with all combinations of all possible readings of the

four runes, I have come up with the sequence s-A-k-R, that is *sAkR* (with palatal umlaut, not indicated), as the only feasible reconstruction. It is the least objectionable paleographically, it alliterates with the following *snArþiR*, and, according to its final *-R*, it represents a word of the same form class as *snArþiR* and *wiltiR*, thus affording the required formal parallelism of the first and second half of line 2.

However, what about the *meaning* of *sAkR*? In attempting to answer this question we move from runic epigraphy and early Norse phonology to a legal aspect of early medieval life in Scandinavia.

There can be no doubt that this runic and early Norse *sAkR* of about 700 A.D. is a cognate of words very extensively attested in later medieval Scandinavian texts of the twelfth century and thereafter. We have Old Norwegian *sækr* (Mod. Norwegian *saka*), Old Icelandic *sekr* (Mod. Icelandic *sekur*), Old as well as Modern Swedish *sakir/saker*. Only in Danish texts, old and new, does no equivalent occur. Nor are there any corresponding formations in West Germanic: Old English *sac*, *sæc* has always been considered a Scandinavian loan; in the Old Germanic dialects on the Continent there is no trace of an equivalent adjective.

In its form the word is clearly an adjectival *-j*-derivative, Proto-Norse **sakjaR*, from a hypothetical PGmc. **sakjaz*, with a host of cognate derivatives and compounds in North Germanic, Germanic, and the other Indo-European languages.

Much more complex is the question as to what precisely was designated by this word—complex because the answer differs not only depending on which Scandinavian dialect or what period of time is involved, but also on whether we refer to its use in legal or nonlegal material. I suspect I am far from having run down all the occurrences of the word in published texts, some of which are not even accessible to me in this area. However, I am inclined to think that the following is a reasonably precise and representative summary of the available facts:

In *Norwegian* the legal texts of the twelfth and thirteenth centuries, such as the *Gulaþingslög*, use *sækr* almost exclusively in the sense of 'obliged to pay a fine'; only very rarely does the word have the more general meaning of 'punishable, guilty'.[2]

The same is true of the *Swedish* legal codifications such as the *Westgötalagh* and the *Östgötalagh* from the end of the thirteenth century; they, too, use *sakir* or *saker* mostly as meaning 'obliged to pay a fine' and only a few times in the sense of 'punishable, guilty'. However, in Swedish another meaning does occur, namely that of 'incriminated'.[3]

Omitting Danish, which fails to attest our particular term, we find in Old Icelandic texts, both legal and nonlegal, the form *sekr*, again—though rarely—in the two meanings which Norwegian and Swedish had in common. However, and this is the significant point: As every reader of the Old Icelandic sagas knows, *sekr* occurs most frequently in the sense of 'outlawed', and, on closer inspection, as referring to (a) the so-called 'full' or

'severe' outlawry (i.e. *proscriptio*), and (b) the so-called 'milder' outlawry (i.e. *expulsio*).

This additional meaning of *sekr*, namely 'outlawed' with its two submeanings of *proscriptus* and *expulsus*, is occasionally found also in legal Old Icelandic usage, as in the *Grágás*. There, however, probably under Norwegian influence, more explicit terms are found to be in use to designate the two kinds of outlawry, namely *skóggangr* for 'proscription' and *fjörbaugsgarðr* for 'expulsion'. And we remember now a passage in the so-called *Grammatical Treatise* on Old Icelandic, the oldest phonemic analysis of a Germanic dialect, where *sekkr* with double *k* and our *sekr* with one *k* are listed in terms of a pair of minimal contrast: *sekkr* (with double *k*) is explained as *ílát*, that is, 'inlet', 'container', 'sack', whereas our *sekr* is identified with *skógarmaðr* (as the more frequent submeaning of the word).[4]

To make it quite clear: while in all Norwegian and Swedish texts, legal as well as nonlegal, from the oldest vellum of the twelfth century to the end of the Middle Ages, *sekr* is never used to designate any sort of 'outlawry', this is precisely the most frequent meaning of *sekr* in Iceland. As we have seen, this is the meaning found repeatedly in the Old Icelandic *Grágás*, and it is, above all, the meaning with which the word *sekr* and its compounds and derivatives occur at innumerable places in the medieval Icelandic family sagas.

It was this *sekr maðr* who more than anyone else in medieval Iceland was feared and suspected of disturbing or ransacking graves. We may think of Gisli the 'outlaw', or of Hörd, who according to his saga breaks into Soti's tomb, who even pulls a ring from the dead man's finger and makes off with boxes full of valuable things. Also the most notorious Icelandic *sekr maðr* of all comes to mind, Grettir, whom his compatriots hesitated to outlaw for fear that he would thus be driven to a life of robberies and extortions.

III

In the light of the impressive testimony of our Old Icelandic sources, both Konrad Maurer and Andreas Heusler have speculated that the attested medieval use of *sækr* and *sakir*, especially in the sense of 'obliged to pay a fine', in Norwegian and Swedish legal terminology represented a later specialization, and that, perhaps owing to colonial conservatism, the attested medieval Icelandic use of *sekr* as referring to 'outlawry' reflected an older meaning of the word.[5]

It also seems reasonable to assume that the subdividing of outlawry into a 'severe' and a 'milder' form, as codified in the *Grágás* and occasionally referred to in the sagas, was likewise due to later specialization and refinement as shown by such relatively recent terms as *skógargangr* and *fjörbaugsgarðr* respectively. In the literary language of the sagas the word *sekr*, nevertheless, on the whole serves as the overall term; only when special explicitness is called for, do we find such amplifications as *sekr skógarmaðr* or *fjörbaugssekþ*.

Now the very fact that Old Icelandic continued to use the term *sekr* for 'outlawry' in general, suggests that at a time before various kinds and degrees of outlawry were set up as institutional penalties to be imposed and pronounced by a constituted body of men, the term *sekr*, or whatever its older form, may have denoted 'outlawed' in a broader sense, namely something like the state of being 'subject to search, pursuit, and killing', all with impunity on the part of those avenging a wrong. As a matter of fact, it may well be that during the time preceding the later institutionalizing of a *de jure* outlawry, there obtained such a practice as *ipso facto* outlawry immediately following upon the commitment of the crime.

Against this background of the actual use of the term *sækr/ sakir/ sekr* in the several late medieval Scandinavian languages and its probable meaning in early North Germanic usage, we shall now briefly review the comparative linguistic analysis of this term and its inscriptional ancestor *sAkR*.

As already indicated, *sAkR* is a *-j*-derivative from Germanic **sak-*, corresponding to a Proto-Indo-European root **sag-* or **səg-*. Thus we may analyze it as an adjectival *-ja*-derivative, let us say, from a noun **sakō*. Or, if we follow the handbooks and most of the existing literature, we can account for the form *sAkR* in terms of what has traditionally been labeled a gerundive or future passive participle, of the type Skt. *yajya*, Greek *hágios*, in the sense of 'verendus'.

As is well known, these formations are particularly frequent in two Indo-European dialects, Old Indic and North Germanic.

In Old Icelandic, which may stand here for early North Germanic as a whole, we have among many others *bærr* in the sense of 'bearable, to be borne' (Skt. *bhāryà*), *drœgr* 'tragbar', *færr* 'fahrbar', *nýtr* 'usable' (OHG *nuzzi*, NHG *un-nütz* with zero grade!), *næmr* '(an)nehmbar', *tækr* 'acceptable', and, specifically in legal terminology, such formations as *dræpr* 'to be killed with impunity', *kirkjugræfr* 'to be buried at a church',—all of these with the lengthening grade of the stem vowel. However, there are others with normal, reduced, or zero grade, such as *gengr* 'gangbar', *-fyndr* 'aufzufinden', *samþykkr* 'agreeable', or *þekkr* 'acceptable' (from *þekkja*). To these we may add our *sekr*.

Interestingly enough, Old Icelandic *sekr* in turn is often found accompanied by a whole string of gerundives, though of a different formation, as in the following quotation from the *Njálssaga* (Ch. 73, 7): "Tel ek hann eiga at verða um sǫk þá sekjan skógarmann, *óælan*, *óferjanda*, *óráðanda* ǫll bjargráð".

As to other derivatives from the root PIE **sāg-* ~ **səg-*, Germanic **sak-* ~ **sōk-*, in North Germanic as well as in other Germanic and Indo-European languages, we find in ONorse the weak verb *saka* in the sense of 'to accuse, blame, harm, scathe', as well as *sekja* 'to sentence to a fine, penalize, punish', and the nouns *sǫk* f. 'offense charged, accusation, suit (in court)' and *sekt* f. 'guilt, penalty', as well as an adjective *saklauss* 'not guilty'.

Corresponding forms in Gothic are the strong verb *sakan*, which translates Greek *mákhesthai* or *epitimân* in the sense of 'to quarrel' and 'to censure' respectively, *andsakan* 'to contradict' (Greek *antilégein*), *un-and-sakans* 'undisputed', the adverb *unsahtaba* 'irrefutably', a noun *sakjo* f., which translates Greek *mákhē* in the sense of 'quarrel, argument', and an adjective *sakuls* in the sense of 'quarrelsome'.

The cognates in Old High German, Old Low German, Old Frisian, and in Old English (as far as the latter has not borrowed from North Germanic) add little but in general corroborate the evidence, in form and meaning, presented by Old Norse and Gothic.

What is more important, is the wealth of forms derived from a root variant with long *-ā-*, PIE **sāg-*, Proto-Germanic **sōk-*, attested in all Germanic dialects and with meanings strikingly close to those of the words already reviewed.

In ONorse there is *sókn* f. used primarily in the sense of 'prosecution' or 'attack', and, of course, there is *sækja* designating 'to seek' and 'to visit' but much more frequently 'to assail', 'to pursue', and especially 'to prosecute', often in combinations like *sækja sǫkum* 'to prosecute in a lawsuit', or *soekja sǫk* in a context like this: '*skalt þú sækja sakir báðir*'.

Gothic likewise has a nominal formation with long *-o-* in *sokns* f. denoting 'controversy, dispute, argument', the adjectival *-j*-derivative *un-and-soks* 'irrefutable', from *andsakan* 'to refute', and the verb *sokjan* with the meaning of 'to seek out, to argue with', forms and meanings which again are largely confirmed by the evidence of West Germanic. One illustration from Wulfila's text (2 Timothy 2, 23) seems to be worth quoting in this connection: '*iþ þos dwalons jah untalons soknins* (*zētēseis*) *biwandei*, *witands þatei gabairand sakjons* (*mákhas*)'. 'Have nothing to do with foolish or ignorant *arguments*; you know they breed *quarrels*.'

Cognates in other Indo-European languages are not plentiful but rather convincing. There is Greek *hēgéomai*, Doric *hagéomai*, OIrish *saigim* 'gehe nach, suche auf', and there are several Hittite forms which Professor Benveniste recognized as representatives of the Indo-European root **sāg-* ~ **sǝg-*, ' qui s'applique aux opérations de l'intelligence pénétrante et calculatrice' (*BSL*, XXXIII [1932], 141). And there is, of course, Latin *sāgīre*, *sagāx* etc. and Cicero's often-quoted definition 'acute sentire', that is, 'to have a keen scent', 'scharf nachspüren'. The meaning of 'to search' or 'to follow intently', often with hostile intent, has certainly taken first place in Germanic.

To come back to the *sAkR manR* of the Eggjum inscription: our summary review of the cognates of *sAkR* in other Germanic and in other Indo-European languages, especially in Italic and Celtic, has led us to the meaning of 'quest, search, pursuit' for a hypothetical Germanic **sak-* ~ **sōk-*, and to the meaning of 'persequendus', that is, 'pursuable as well as to be pursued', for the adjective *sAkR*. Such a circumstance, whether it was an *ipso facto* state of being 'exposed to search and pursuit', immediately following the commitment of a crime, or whether it was a sentence resulting

from legal action, would drive a person to take to the woods and to the wilderness. Small wonder, then, that in the Eggjum inscription a *sAkR manR* is mentioned among those who were feared for ransacking graves, no less than the *snArþiR* and *wiltiR* men referred to in the second half of the same line, and no less than the *de jure* outlaws that haunt the scene of the Icelandic family sagas.

IV

The assumption that the early eighth century phrase *sAkR manR* denotes a 'person pursuable and to be pursued' (with impunity on the part of those avenging a wrong), is supported by another bit of inscriptional evidence, this time from Sweden and from the early tenth century.

In a runic inscription discovered as late as 1930 near Oklunda in Southern Sweden and first published by Arthur Nordén in the subsequent year,[6] the word *sakiR* occurs in the following context.

> 'Gunnar carved these runes. And he fled *sakiR*. And he took refuge in this sanctuary (*vi*).' The next line is not quite clear, but probably means: 'And he has had his guilt settled (?), and he bound it (namely: the settlement, that is to say, he made it binding on him).' To this has been added, quite separately and running along the right-hand margin: 'Vi-Finn carved this.'

What we learn from this inscriptional document is that a man by the name of Gunnar, presumably because of some serious crime, has become fugitive. It is in this state that he is called *sakiR* in early tenth century Swedish. Gunnar has then claimed asylum in the sanctuary near Oklunda. What follows is less important for our discussion, namely that most probably some kind of settlement was reached between him and the wronged party, a settlement which may have been attested, as it were, by Vi-Finn, the man in charge of the *vi*.

Now it so happens that this isolated bit of inscriptional evidence is corroborated by a lengthy section in the thirteenth century legal code of the island of Gotland, the so-called *Guta lagh*. The particular paragraph specified that a guilty person who had taken refuge in such an asylum had a year's grace to make a settlement; if no settlement was brought about, and only then, the guilty person would officially be declared an 'outlaw', a state which in the Swedish of the late Middle Ages was designated by the word *friðlös* or the much-debated and still obscure *biltogh*.[7]

There is no doubt that Gunnar's state immediately upon his commitment of the crime, his state of being *sakiR*, as it is called in the tenth century Swedish inscription of Oklunda, cannot be identified with the officially imposed, *de jure* outlawry, a penalty inflicted only under certain circumstances. On the contrary, it is precisely Gunnar's avoidance of such *de jure* outlawry—by virtue of making a settlement after he has become *sakiR* but has found temporary refuge in a sanctuary—that has occasioned the preliterary document of Oklunda and, as far as WE are concerned, has

supplied us with additional inscriptional evidence of the early North Germanic meaning of *sAkR/sakiR*. It now seems more probable that not only in the tenth century Old Swedish inscription of Oklunda but also in the eighth century Old Norwegian inscription of Eggjum the term *sakiR/sAkR* did mean 'subject to pursuit' (with impunity for the pursuers); it also seems likely that in both instances a kind of *ipso facto* outlawry was implied.

V

Lest I be misunderstood, perhaps I should make one more point in closing. The fact that we have found inscriptional evidence of the same legal, or, if you prefer, prelegal term and very probably with the same meaning in both early Norwegian and early Swedish—the fact that the term may at one time have existed in early Danish, to judge by the Scandinavian loan *sac*, *sæc* in Old English, and that also Icelandic, though much later, presents this term and in a similar, though, let us say, institutionalized function—all this would seem to suggest that we are dealing here with a term of ancient and common North Germanic origin, with a historico-legal *Erbwort* shared by all of Scandinavia as a result of *Urverwandtschaft*. However, as long as our knowledge of *early* North Germanic legal terminology and institutions remains as spotty as it is, we cannot rule out the alternative of explaining what we have found concerning the form and meaning of *sAkR/sakiR* in the several North Germanic dialects as due to mutual borrowing or imposition, which characterize so much of the legal history of the Scandinavian peoples.

I say this rather pointedly because of the prevailing tendency of ascribing every legal term and function attested for two or three branches of Scandinavian to Proto- or Common North Germanic. The cause célèbre *against* this long-practiced methodology is the office of the so-called 'lawspeaker' which Heusler as late as 1911 and lesser lights even today hail as 'gemeinnordisch'—in spite of the fact that the great Konrad Maurer in a paper read before the Munich Academy of Sciences anno 1887, had delivered a carefully documented and resounding 'no' to what he branded 'das angebliche Vorkommen des Gesetzessprecheramts in Dänemark'.[8]

As I have suggested, considerations of facts and of probability seem to favor, in the case of *sAkR/sakiR*, the assumption of a common Proto-North-Germanic **sakjaR* with the proposed meaning as the common antecedent of the forms and meanings which we subsequently encountered in the individual early Scandinavian legal terminologies.

On the other hand, there is quite obviously no reason whatever for assuming a common Proto-Germanic **sakjaz* with a similar meaning as the common ancestor of corresponding terms and meanings in *all* Germanic languages, that is, not only in North Germanic but also in Gothic and West Germanic as well. In Old English the form *sac*, *sæc*, as indicated, has always been explained as a loan from North Germanic; in Old High and Old Low German as well as in Gothic we found no trace of a form corresponding to the North Germanic adjective *sAkR/sakiR*.

However, we do know of *other* terms which refer to some form of 'outlawry', such as *útlaga* in Old English. Yet, again, as everyone knows, *útlaga* is not an *Erbwort* but another Scandinavian intrusion in early medieval English.

There also is an Old English term *flíema*, which denotes a person fugitive because of a crime and which therefore has often been thought to refer to a person sentenced to outlawry, in spite of F. Liebermann's warning that *flíema* means nothing but 'den tatsächlich zum Walde entflohenen Verbrecher'.[9]

The same is true of Old English *wealdgenga* 'wood-going man', a term rarely found but frequently quoted as a supposed parallel of the Scandinavian *skógarmaðr* and as additional evidence for a Proto-Germanic concept of 'Waldmann' for the outlaw.

For the same reason, much has been made of the phrase 'homo qui per silvas vadit', cited from the Merovingian *Edictus Chilperi* of the sixth century A.D., although the very learned legal historian Franz Beyerle in 1915 and, again, quite recently and independently, Hinrich Siuts have made it very clear that the context of the particular passage excludes any idea of outlawry.[10]

In Old High and Old Low German vernacular texts we find nothing but the terms *âhta* and *âhtjan*, MHG *âhte, æhten, æhter*, etc. referring to 'outlawry', terms which in this sense are restricted to the Continent.

In short, legal historians and, of late, also linguists in the Germanic field have come to agree that there is no such thing as one term for 'outlawed' which all of the Germanic dialects at one time had in common, and that therefore there is no ground for positing or reconstructing one common Proto-Germanic term.

Again, I say this rather pointedly because it seems to me that in historico-legal research done by Germanists (who in this case may have acted like Romanists in reverse!)—more than in any phase of "thing-and-words" research—an indiscriminate faith in *Stammbaum*-inspired reconstruction of common 'proto-words' and, consequently, of common 'proto-things', as supposedly attested by these words, has persisted to the present day. And this methodological overconfidence has been coupled with the romantic notion inspired by Grimm and others as to the extraordinarily old age of all attested legal terms and institutions in Germanic.

Notes

[1] M. Olsen, *Eggjum-Stenens Indskrift med de ældre runer* (Christiania, 1919). L. Jacobsen, *Eggjum-Stenen* (Copenhagen, 1931). A. Heiermeier, *Der Runenstein von Eggjum* (diss. Berlin, 1933). W. Krause, *Runeninschriften im älteren Futhark* (Halle, 1937) 101–10; 2nd ed. (Göttingen, 1966) 227–235.

[2] G. Storm og E. Hertzberg, *Norges gamle Love indtil 1387, V:Ordbog* (Christiania, 1895) 544.

[3] C. J. Schlyter, *Glossarium ad Corpus juris Sueo-Gotorum antiqui = Ordbok till Samlingen af Sveriges Gamla Lagar* (Lund, 1877), s. v. *saker*.

[4] E. Haugen, *First Grammatical Treatise: The Earliest Grammatical Phonology (Language Monographs*, 25) (Baltimore, 1950), p. 28: *secr er skógarmaðr. . . .*

[5] K. Maurer, *Altisländisches Strafrecht und Gerichtswesen* (*Vorlesungen über altnord. Rechtsgeschichte*, 5.) (Leipzig, 1910), 137–74. A. Heusler, *Das Strafrecht der Isländersagas* (München und Leipzig, 1911) 124ff.

[6] A. Nordén, Ett rättsdokument från en fornsvensk offerlund: Oklundaristningen, en nyupptäckt östgötsk rökrune-inskrift. *Fornvännen* XXVI (1931) 330–51. Cf. also O. v. Friesen, *Runorna* (*Nordisk Kultur*, VI) 152f.

[7] Cf. *Lex Gotlandiae, Svetice et Germanice*, ed. E. Wessén (*Corpus Cod. Svec. Med. Aevi*, V) (Copenhagen, 1945), no. 13.

[8] K. Maurer, *Sitzungsber. der Akad. d. Wiss. zu München, Philos.-philol. und Hist. Cl.*, 1887 (München, 1888).

[9] Die Friedlosigkeit bei den Angelsachsen, in *Festschrift f. H. Brunner zum 60. Geburtstag* (Weimar, 1910) 24.

[10] Fr. Beyerle, *Das Entwicklungsproblem im germanischen Rechtsgang* (*Deutschrechtliche Beiträge*, X. 2); (1915) 39, Anm. 62. H. Siuts, *Bann und Acht und ihre Grundlagen im Totenglauben* (Berlin, 1959) 25.

[11] A similar scepticism has recently been expressed by K. von See in his exemplary reexamination of a number of Old Norse legal terms, *Altnordische Rechtswörter* (Tübingen, 1964). While neither *sAkR/sakiR* nor any other terms referring to 'outlawry' have been dealt with in this monograph, we may expect further studies including the terms of outlawry by the same author, cf. op. cit. 140.

Some Widespread Indo-European Titles

Werner Winter

In this paper, a number of titular terms will be discussed. Each one of these occurs in more than one Indo-European language and is therefore to be considered for possible inclusion in the reconstructed lexicon of at least some subgrouping within the whole of Indo-European. The discussion will center on matters of form and quasigeographical distribution; in some instances attempts will be made to arrive at statements about the function of a specific term. Three topics will be treated: (1) the distribution of Lat. *rēx* and forms corresponding to it; (2) the distribution of Gk. *despótēs* and related forms; and (3) Gk. *ánax* and related titles.

1. The facts about the set of forms represented by Lat. *rēx* are well known except for a few details. A perfect match to the Latin word is found in Gaul. *-rīx* (with Germanic forms borrowed from Celtic) and in OInd. *rā́ṭ*. With a deviation in morphological structure, OInd. *rā́jā* and Khotanese Saka *rrāysan-* can be added; Gk. *arēgṓn* 'helper', even if it should be parallel to the Indic and Iranian forms, remains outside the area of our immediate interest, since it is not a title with a reasonably well-defined function. By the same token, Thrac. *Rhêsos*, being a proper name whose exact interpretation remains in doubt, is to be disregarded.

Several problems concerning matters of detail deserve our attention. The first of these is the question of how the formal relationship between OInd. *rā́ṭ* and OInd. *rā́jā* is to be described. One approach, favored by Mayrhofer, takes *rā́ṭ* as an inherited form from which *rā́jā* is held to be relatively independent: it is derived from a Vedic abstract noun, *rājáṇ-*, thought to mean 'Leitung' and equated with Av. *rāzan-*, a noun of rather uncertain denotation; the feminine *rā́jñī* 'queen' is then interpreted as an *-ī* extension of the masculine *-n*-stem. As an alternative, Mayrhofer lists Wackernagel's proposal to consider *rā́jñī* an inherited form corresponding exactly to OIr. *rīgain* and, not quite so exactly, to Lat. *rēgīna* (the latter possibly reshaped from **rēgnīs* after *domina*); *rā́jā* would then be a form derived from the feminine extension of *rā́ṭ*. Wackernagel's interpretation of the data seems preferable because of the perfect parallelism found among OIr. *rī*:*rīgain* ::Lat. *rēx*:**rēgnīs*::OInd. *rā́ṭ*:*rā́jñī*. The relationship between the masculine and feminine forms would match that found in OInd. *pátiḥ* : *pátnī*, and this derivational paradigm 'master':'mistress', whose antiquity is beyond doubt, may indeed have provided the pattern after which the paradigm

'king' : 'queen' was shaped. The postulated development of the masculine form *rā́jā* from the feminine *rā́jñī* by retrograde derivation (back-formation) may have a close parallel in Slavic *pan* 'master' beside *pani* 'mistress', although the communis opinio prefers a quite different interpretation of these forms: *pan* is generally believed to have been shortened from *župan* 'lord over a region', found in several Slavic languages, or from **gŭpanŭ*, attested only by Old Czech *hpan*; *župan* in its turn is analyzed as an *-n-*extension of *župa* 'region'. *Župa*, however, is without a persuasive etymology, and it may therefore be proposed to identify *župan* as a compound whose first part would be related to Goth. *gawi* 'region', and whose second part would be an *-n-*extension of the stem found in Lat. *potis*, etc. *Pani* then would be a form exactly parallel to Gk. *pótnia*, OInd. *pátnī*. *župa* 'region' of course would be nothing but a back-formation from *župan*.

If there is reason to think that OInd. *rā́jā* is a form much more recent than *rāj-* in OInd. *rā́ṭ*, the forms from Khotanese Saka take on considerable importance for an assessment of the chronology of this development: is it perhaps to be ascribed to Proto-Indo-Iranian? Or, if the development of the *-n-*stem in Iranian was an independent phenomenon, can we now posit an Iranian **rā́z* 'king'? In view of the relatively late attestation of the Saka forms and of their geographical proximity to Indic, an alternative interpretation of the data seems to have greater likelihood of being justified: it seems better to reckon with the possibility (and, so it would seem, a very strong possibility indeed) that KhS *rrāysan-* 'king', *rräspūra-* 'king's son', *rräysdūtar-* 'king's daughter' represent not inherited forms of great antiquity, but rather the results of a borrowing process, in the course of which Indic *-j-* was replaced by KhS *-ys-*, i.e. [z], its etymological equivalent. Such reshaping of loans is common enough when borrowing takes place between two relatively closely related languages or dialects: if it is possible for a native speaker of the one language to recognize equivalents to forms of his own language in the other one, abstracting of sound correspondences can, and often will, take place.

Porzig takes the distribution of the terms for 'king' that have been discussed here as evidence indicating survival of a highly archaic term in marginally located languages. Such an assumption of course has some interesting implications: We are forced to conclude that the item **Xrēǵ-s* 'king' occurred, in a more remote past than that represented by our documents, in a much larger part of the Indogermania—more specifically, in the regions intermediate between the areas of the Pre-Celts and Pre-Italics on the one hand and those of the Pre-Indians on the other (whatever that may mean in terms of actual places on a map of Eurasia). From historical sources we know that at least in some of the regions where the term is not found (and where, if Porzig and his predecessors are right, it may have been lost), the institution of a permanent ruler for war and peacetime alike did not exist; this certainly applies to Germanic and Slavic groups in their respective homelands. If permanent kingship (no matter how small the region ruled by a 'king' may have been), and not just the institution of a

warlord elected for a limited time, was a rather widespread Indo-European cultural phenomenon, then there is some likelihood that we have to assume that Indo-European speaking groups settling in north central Europe gave up this social trait. If so, why did they do it? We may conjecture two possible causes: Either this was brought about by contacts with people of a quite different cultural background, or by special conditions prevailing in these northern regions that were not found in other parts of the Indo-Germania. If interaction with a different culture took place, this may of course have happened after the ancestors of the groups later speaking Celtic, Italic, and Indic languages had become separated from the rest, even though severe difficulties remain in view of the otherwise apparently rather close contacts between Celtic and Germanic groups. On the other hand, if outward conditions played a decisive part, it seems very difficult to consider the historical homeland of Germanic and Slavic tribes the homeland of the entire Indo-European group, and we would somewhat unexpectedly gain an additional argument in favor of the assumption that the area of the least extension of the Indo-European group should be sought to the south of what is now Germany and Poland. Obviously the second line of reasoning has a certain attractiveness as it seems to offer more tangible results; we may even want to carry the argument about the implications of the inherited term for 'king' somewhat further: groups like those that settled Greece may have lost not only the term, but also the institution of permanent kingship, and may have adopted it again once they came into contact with people of the Mediterranean area. That they, and their probable predecessors in Greece, Georgiev's Pre-Greeks, knew the institution of the warlord seems indicated by the presence of the terms *koíranos* and *túrannos* (related to Goth. *harjis* 'army'); the normal Greek term for 'king', *basileús*, remains without a persuasive etymology and may therefore well be a loan word from a source still unknown.

2. Forms closely related to Gk. *despótēs* 'lord' have been adduced from Old Indic (*dámpatiḥ*) and Avestan (*dəng paitiš*); the purpose of the following remarks is to show that a parallel form survived in Armenian, thus adding to the inventory of isoglosses between these southeastern Indo-European languages. The argument to be presented will have to be somewhat less direct than would be desirable.

Armenian seems to share with Balto-Slavic and Indo-Iranian a special treatment of PIE *s* after **i *u *r* (**k*?). We find PIE *rs* reflected by Arm. *ṙ* (/rr/) in *oṙk'* 'buttocks': Gk. *órros*, OHG *ars*, *moṙanan* 'they forget': Toch. A *märsneñc*. PIE *us* seems to yield Arm. *ər* in the nominative singular of *u*-stem adjectives like *barjr* 'high': Hitt. *parkus*, *t'anjr* 'thick': Lith. *tánkus*.

For PIE *is*, we would expect to find Arm. *ir* or *ər*, depending on the position of the stress. A good case can readily be adduced: the numeral 'two' in Armenian clearly has undergone some secondary change: *erku* seems to show the results of the transfer of initial *er-* from *erek'* 'three' with regular *er-*. The expected initial *k-* from **dw-* occurs in just one form

with a denotation related to 'two': one of the Armenian equivalents of 'twofold' is *krkin*. The comparison with *erek'kin* 'threefold', (*čorek'kin*) 'fourfold', etc., shows that *-kin* is the second element of a compound, the first part of which is the full form of a numeral. *kər-* in *krkin* cannot be derived from **dwōw* or any similar form; it can, however, be equated exactly with Lat. *bis*, Gk. *dís*, OInd. *dviḥ* 'twice'. The equivalent of Gk. *trís* 'thrice' is found in a related adverbial formation: *erraki* 'trebly', derived from unattested **erir* (which may have been lost because of homonymy with *erir*, the short form for 'third'); the corresponding form 'doubly' is based not on the simplex **kir*, but on the extended form *krkin*: we find not **kraki*, but rather *krknaki*.

The observations just made about the origin of *krkin* enable us to analyze Arm. *tēr* 'lord'. Descriptively speaking, *tēr*, genitive *teaṙn*, can safely be taken as a compound containing an element *ti-* followed by *ayr*, genitive *aṙn* 'man', particularly since the word for 'mistress', *tikin*, can be broken down into *ti-* plus *kin* 'woman, wife'. The origin of *ti-* has thus far remained obscure; *kin* corresponds precisely to Slav. *žena*, etc., and *ayr* has always been taken to match Gk. *anḗr*. The latter explanation has a severe handicap: while the genitive of *ayr*, *aṙn*, agrees perfectly well with Gk. *andrós* or *andrí*, the development of **-nēr* to *-yr* and not to **-nir* > **-nər* remains incomprehensible. Regular sound developments seem precluded; two possible alternatives can be suggested: either *ayr* resulted from the interference of such forms as *hayr* 'father' and *elbayr* 'brother', or *ayr* does not reflect **Anēr* at all, that is, the paradigm of *ayr* is suppletive. The second alternative seems preferable as it provides us with a chance to analyze *tēr* without any residue remaining unaccounted for: Proceeding from a Proto-Indo-European form **dem-potis* as reflected by OInd. *dámpatiḥ*, a Proto-Armenian form, showing the effects of the Great Armenian soundshift, the development of **e* before nasal becoming *i* (cf. *hing* 'five'), of Arm. *a* from PIE *o* (cf. *akn* 'eye', *ac'k'* 'eyes'), and of Arm. **-ir* from PIE *-is* (cf. *krkin*), would be **tiN-faθir*. Before a spirant, the nasal was lost (cf. *us* 'shoulder', *arcat'* 'silver'); the spirant *f*, now in intervocalic position, was represented by an allophone which fell together with the reflex *y* of intervocalic *θ*; unstressed *i* in the first and the last syllable was syncopated. The resulting form **tyayr* then was the immediate source of *tēr*, the form actually attested. From **tyayr*, a first element *ti-* was extracted to form *tikin* 'lady'; after *ayr* and *aṙn* had been combined in one paradigm the oblique forms of *tēr* could become adjusted to those of *ayr*. The fact that *tēr* occurs in composition with the word for 'house' shold not be held against the proposed etymology: Arm. *tanutēr* 'master of the house' is in one category with Gk. *oikodéspoina* 'mistress of the house'—in both cases the old compound could no longer be analyzed as to its constituent parts.

A few comments may be added concerning the vocalism of **potis*. It seems impossible to ascribe the vocalism of the first syllable to the former presence of an *o*-coloring laryngeal, since in view of OInd. *píbati*, etc., one would expect to find **b-* rather than **p-*. On the other hand, a primary

-o-vocalism appears unlikely. A way out of the dilemma may be offered by the very forms under discussion: it could be that the *o*-vocalism developed in the second stem position of a compound (cf. Gk. *patḗr*:*apátōr*), and later on was transferred to the independently used form. If the assumption is correct, Gk. *déspoina* is in some respects more archaic than Gk. *pótnia*, though of course the free use of the decomposed form can be projected back very far. Still, it remains noteworthy that in several Indo-European languages **potis* is used predominantly or exclusively in compounds—cf. Gk. *despótēs*, Goth. *brupfaps* 'bridegroom', OPruss. *waispattin* 'mistress', OCS *gospodĭ* 'lord' (if the etymology proposed above for Slav. *pan* is correct, it may be suggested that the compound *župan* also may be more archaic than the simplex).

The area in which Gk. *despótēs* and its equivalents were used agrees reasonably well with that in which the root noun **dem-* 'house' was found. Where only the extension in *-u* occurs, as in Latin or Slavic, the extended stem is not used as the first stem of a compound. Whether this fact is to be interpreted as an indication that upon the development of the extended stem a compound containing the mere root as its first stem was eliminated, or whether the compound **dem(s)-potis* never existed outside the south-eastern group of Indo-European languages cannot be decided.

3. Gk. *ánax*, older *wánax* 'lord' has generally been considered a word borrowed from a source unknown. The strikingly similar Phrygian form *vanaktei* has been taken to be a loan from Greek. The comparison with Toch. B *ñakte* 'lord' is properly rejected by Pedersen.

Supposing, for the sake of the argument, that Gk. *wánax* was an inherited form, what could be reconstructed as the underlying Proto-Indo-European form? Purely in terms of internal reconstruction, one would come up with a possible source form **wnAk-* for *ánakes* 'Dioxcures' and *ánassa* 'lady' and **wnAkt-* for the oblique forms of *ánax*, with the source of the nominative uncertain. The reconstructed forms have a shape strangely different from normal Proto-Indo-European patterns, and one is therefore tempted to accept the assumption of foreign origin in Greek as confirmed by more general considerations.

There is, however, a fact to be taken into account which has hitherto been overlooked. Gk. *ánassa* 'lady' has what appears to be a perfect match in Toch. A *nāśi* 'lady':*nā-* may be from older **wnā-* (cf. *lānt*, accusative of *wäl* 'king'), and this is either from PIE *wna-* or PIE *wnX-*; the form is a feminine derived by means of a suffixed reflecting PIE *-yA* from a stem ending in a palatal/velar/labiovelar that cannot be further identified. Admitting that uncertainties necessarily remain, one has to state that there is no argument against considering Gk. *ánassa* and Toch. *nāśi* an exact and complete formal and semantic match.

The masculine equivalent of Toch. A *nāśi* is *nātäk* 'lord'. We cannot show that the sequence *-kt-* was subject to metathesis in Tocharian; we can therefore not derive Toch. A *nātäk* from **wnAkt-* as we could Gk. *wanakt-*. However, it is a well-known fact that in Greek *-tk-* became *-kt-* by meta-

thesis—cf. *tíktō* : *étekon*; hence it would be possible to derive both Gk. *wanakt-* and Toch. A *nātäk* from a common source from **wnAtk-*.

This common source form deviates in more than one way from the canonic patterns of Proto-Indo-European; in particular, the relationship between the reconstructed masculine and feminine forms (**wnAtk-*:**wnAk-*) is exceedingly strange. The question then arises again whether we are confronted here with non-Indo-European linguistic material; but now this question has become more interesting by the possibility that the borrowing process led to the inclusion of the same loan word in more than one language. Does this mean that the transfer was undertaken by a Graeco-Tocharian group? Or through Pre-Greek into Pre-Tocharian (or vice versa)? Or independently, but from the same source? Nothing seems to favor the first choice; but even the next two would be interesting enough as they would place Pre-Greeks and Pre-Tocharians relatively close to one another. And the further question arises: How early would such a borrowing process have taken place? The Greek forms are attested from Mycenean onward; but are we justified in using transcriptional devices implying that the forms were borrowed when laryngeals were still separate entities in Pre-Greek and Pre-Tocharian?

I shall not try to answer these questions. The data do not seem to provide a basis for a sound decision one way or the other. Still, it seemed useful to point out this series of agreements between Tocharian and Greek, agreements too detailed and too complex to make it likely that what we have before us is mere coincidence—nothing but a mirage created by chance. But then, even though it seems less probable, the possibility of course has to be kept in mind that the formal and functional identicality of the Tocharian and Greek words may have no significance whatsoever for our work on the reconstruction of Proto-Indo-European, its dialects, and their interrelation.

Germanic and Regional Indo-European (Lexicography and Culture)

Edgar Polomé

The problem of the position of Germanic among the Indo-European languages has often been discussed, especially with regard to the lexical correspondences between Germanic and neighboring Indo-European dialects. As early as 1858, C. Lottner[1] listed no less than 137 such Latino-Germanic isoglosses, pointing out 'dass die lexicalischen verhältnisse dem aus grammatischen und culturgeschichtlichen gründen gewonnenen resultat nicht widersprechen.' He did, indeed, especially stress the culturally important correspondences in the field of agriculture and law, e.g. names of cereals like Goth. *atisk* 'grainfield', OHG *gersta* 'barley', and Goth. **baris* 'barley' corresponding to Lat. *ador* 'spelt', *hordeum* 'barley', and *far* 'spelt, coarse grain'; the technical term OHG *furihha* 'furrow': Lat. *porca* 'ridge between two furrows'; the 'staatsrechtlicher Ausdruck' Goth. *gamains*: Lat. *communis*; the double meaning 'hand' and 'legal authority' of Lat. *manus* also reflected by ON *mund* 'hand' and OHG *munt* 'tutelage'; the term for 'law' itself, i.e. Lat. *lex* (gen. *legis*): ON *lög* (n. pl.); etc.[2] However, Lottner's aim was essentially to disprove the current theory of closer relationship between Greek and Latin, and he did not actually give the problem of the specific position of Germanic versus Balto-Slavic any special consideration. The idea of a much closer link between these two dialects originated in the writings of K. Zeuss[3] and J. Grimm and was backed up by A. Schleicher:[4] though lexical correspondences were taken into consideration, the main argument was a set of alleged common innovations on the morphological level. It took the careful perusal of the nominal and pronominal inflection system of Balto-Slavic and Germanic by A. Leskien to show that none of the arguments advanced in favor of their closer relationship on the basis of their inflections was cogent, except for the use of *-m-* in case-endings where other languages show *-bh-*.[5] However, the negative result of this study did not discourage the supporters of Schleicher's assumption of a Germanic-Baltic-Slavic subgroup of the Indo-European dialects. The same year as Leskien's book, a monograph by R. Hasselcamp, entitled: 'Ueber den Zusammenhang des lettoslavischen und germanischen Sprachstammes' was published in the same series of the Jablonowski Society in Leipzig, but henceforth the attention was con-

centrated on lexical correspondences, as evidenced by the doctoral dissertation of C. C. Uhlenbeck: 'Die lexicalische Urverwanduschaft des Baltoslavischen und Germanischen' (1890). After stressing in his article: 'Die Stellung des Germanischen im Kreise der verwandten Sprachen', published in the *Zeitschrift für deutsche Philologie*, 29 (1892) 289–305, that there is nothing in the phonological and morphological systems of Balto-Slavic and Germanic which could point to a closer relationship, Herman Hirt briefly discusses the lexical correspondences listed by J. Schmidt,[6] F. Kluge,[7] and P. Kretschmer[8] and concludes: 'Alles was ich gefunden habe, sieht wie ganz zufällige Übereinstimmungen aus. Kulturworts, bestimmte Kategorien fehlen ganz.' (p. 295) He furthermore considered the *satəm*:*centum* dialect contrast established by von Bradke as a major argument against any closer relationship of Germanic and Balto-Slavic.[9] Therefore, he proceeds to a re-examination of the Latino-Germanic isoglosses listed by E. Lottner and adds an impressive series of morphological correspondences to the 117 lexical correspondences he retains. His conclusion remains, however, quite prudent: 'Mir liegt es fern, aus diesen Tatsachen etwa eine nähere Verwandtschaft des Italischen mit dem Germanischen ableiten zu wollen, obgleich eine Nachbarschaft der beiden Volksstämme auch aus allgemeinen Gründen keine kühne Annahme wäre. Ich wollte nur zeigen, dass von der Verwandtschaft des Litu-slavischen mit dem Germanischen nichts zu halten ist' (p. 304). The impact of his views was strong as appears from the statement of F. Kluge in *Urgermanisch-Vorgeschichte der altgermanischen Dialekte* (3rd ed., 1913, 38): 'Aber so viel Einzelheiten man auch beibringen mag, in denen Slavolettisch und Germanisch miteinander übereinstimmen, so besteht doch bei vielen derselben der Verdacht der Möglichkeit von Entlehnungen hinüber und herüber . . . Die . . . beigebrachten Übereinstimmungen zwischen dem germ. und dem lat. Wortschatz sind beweiskräftiger für die Möglichkeit einer näheren Verwandtschaft zwischen Germanisch and Lateinisch. Aber . . . eine nähere Urverwandtschaft zwischen Germanisch und Slavisch hat heute kaum noch Vertreter, seitdem man die Gliederung des Indogermanischen in *centum*-Sprachen und *satem*-Sprachen für durchschlagend hält . . .' More negative even is W. Streitberg's attitude, when he describes Germanic as 'ein durchaus selbständiges, den übrigen idg. Einzelsprachen koordiniertes Glied der idg. Sprachfamilie', because 'bis jetzt (sind) alle Versuche missglückt, *n ä h e r e* Verwandtschaft zwischen ihr und einer der andern idg. Sprachen nachzuweisen.'[10]

With the new approach initiated by A. Meillet in his challenging monograph *Les dialectes indo-européens* (1908), the whole problem of the position of Germanic needed thorough revision: since Italic (i.e. Latino-Faliscan, Osco-umbrian, and the lesser Italic dialects) and Celtic were supposed to have been so closely associated as to constitute a coherent linguistic community at the oldest period of their individualization within the Indo-European area, the correspondences between Italic and Germanic as well as those between Celtic and Germanic which had been repeatedly pointed

out since the days of K. Zeuss and H. Ebel, had to be appraised in the light of this new theory. Besides, the study of the isoglosses running through the Indo-European territory had considerably reduced the importance of the *centum* : *satəm* contrast, and Balto-Slavic, now considered as a close linguistic community, shared more than the case-endings in *-m-* with Germanic: 'la ligne du traitement de **o* et celle de la chute de **ə* intérieur par example montrent que ces trois langues sont issues de parlers indo-européens qui présentent certains traits de ressemblance.' Actually, a closer study of lexical items shows that the northwestern Indo-European dialects, i.e. Slavic, Baltic, Germanic, Celtic, and Italic, share a considerable number of terms reflecting a definite common cultural development, to which many of the formerly quoted isoglosses, e.g. between Latin and Germanic, directly refer. Lat. *far* (gen. *farris*) and *farīna*, with Umbr. *farsio* **fasiu**. *farrea*, which had been compared with ON *barr* 'barley', Goth. *barizeins* 'of barley', is also connected to OCS *brašĭno* 'food', Serb. *brȁšno* 'flour', Russ. *bórošno* 'rye-flour', so that the whole set may reflect a Northwestern IE term designating 'food derived from cereals'.

Meillet's work marked a turning point in the study of the interrelationship of the Indo-European dialects. Applying the findings of dialect geography to the results of the splitting of the Indo-European community, Italian linguists tried to throw additional light on the various degrees of relationship of the Indo-European dialects and on their relative chronology. Collecting an impressive number of phonological, morphological, and lexical correspondences, G. Bonfante came to the conclusion that Italic, Celtic, and Germanic constituted a close Western group, whereas Indo-Iranian and Slavic formed as compact an Eastern group, with Baltic as a kind of transition between both groups in the North, though it remained closer to the Eastern group.[12] Taking into account the valid objections made against Meillet's Italo-Celtic theory as well as Devoto's strong argument in favor of the relatively late development of Latin and Osco-Umbrian correspondences, V. Pisani situated Germanic at the Northern outskirts of the IE area, between Celtic and Balto-Slavic, with Latin and Illyrian to the South, the latter separating Germanic from Osco-Umbrian.[13] Chronologically, however, there were practically no linguistic relations between the Celtic and Germanic areas before the breaking up of the Indo-European unity, so that Pisani presumes that they must have been separated at an earlier date by a political and perhaps also religious boundary.[14] Reexamining the evidence on Germano-Balto-Slavic linguistic relationship, he then reaffirms, against Hirt, that, after the moving out of the pre-Celtic group, a considerable nucleus of Indo-Europeans who would later constitute the Germanic and Balto-Slavic tribes, formed such a unity that they shared quite a series of innovations, while the area was also subdivided by smaller waves accounting, e.g. for Germano-Baltic correspondences.[15] Taking over the results of Krahe's inquiries on Illyro-Germanic and Illyro-Balto-Slavic correspondences, Pisani ultimately regroups Germanic, Balto-Slavic, and Illyrian (to which he adds Thracian) into a 'central' sub-

group of the Indo-European community,[16] which he contrasts with the 'western' group consisting of Italic and Celtic, the 'southern' group, including Osco-Umbrian, Greek and Macedonian, and the 'eastern' group, limited to Indo-Iranian. Apart from the *-m-* case-endings, which Illyrian and Thracian do not share with Germano-Balto-Slavic anyhow, the non-lexical material adduced is, however, far from convincing; e.g. the de-development of [t] in the cluster /sr/: this also occurs in Brythonic, and in Slavic it is younger than the assimilation of */ḱ/, since */ḱr/ is reflected by *str*, whereas in Germanic the change occurs prior to the operation of Verner's law. Therefore, Pisani's views even failed to convince his Italian colleagues: in an important contribution to the *Hirt-Festschrift*,[17] G. Devoto resolutely reaffirms the closer links between Germanic and Latin and between Germanic and Osco-Umbrian, but points out that, while Proto-Germanic with Proto-Celtic and Proto-Latin belongs to West-Indo-European, it also contrasts together with Proto-Slavic and Proto-Baltic, as North-Indo-European, with South-Indo-European including Latin. Moreover, he stresses that, while the Celtic and Latin cultural vocabulary points to a prevailingly priestly-aristocratic social organization, the corresponding Germanic and Osco-Umbrian lexicon rather implies a more democratic type of society, as evidenced by the apparent absence of **teutā* in Latin and of *rēx* in Osco-Umbrian and Germanic. This was further confirmed by G. Bonfante, who collected 21 morphological and 46 lexical correspondences between Germanic and Osco-Umbrian.[18] Unfortunately much of the material adduced by the latter remained rather disputable, as was also the argumentation of the 'neolinguistic' school in favor of the extreme conservatism of Germanic.[19] As a consequence, when W. Porzig re-examined the relations of Germanic with the neighboring IE dialects in his book *Die Gliederung des indogermanischen Sprachgebiets* (1954), he carefully screened the material accumulated by his predecessors and was especially concerned with the validity of the adduced phonological, morphological, and lexical isoglosses as indications of dialectal variation. As regards Germanic and Italic, he listed three phonological correspondences: (1) the change of the voiced aspirates into voiced fricatives, (2) the back vowel reflexes of syllabic /l/ and /r/,[20] (3) the change of **-tt-* to **-ss-* through **-ts-*, the first two of which can be called in question, since it is more likely that (1) the voiced aspirates became voiceless aspirates in Latin and Osco-Umbrian as in Greek;[21] (2) the reflexes of IE [r̥] and [l̥] were vocalic resonants rather than sequences of *u* plus resonant in Proto-Germanic,[22] whereas to claim an actual correspondence between the Germanic and Latin treatment of IE [r̥] and [l̥], Szemerényi's hypothesis that the syllabic 'liquids' are reflected by *ul* and *ur* rather than by *ol* and *or* in Latin[23] should be unconditionally accepted. As for the treatment of IE dental plus **t* in intervocalic position, it is probable that the intermediate stage **-tst-*, preserved in Anatolian, was reduced to **-ts-* in Celtic as well as in Latin, Osco-Umbrian and Germanic, with further assimilation of **-ts-* to **-ss-*: the Celtic forms in which **-st-* is assumed to reflect IE **-tt-*[24] do not, indeed,

constitute any more conclusive evidence than the Germanic forms in *-*st*-, on which R. Kögel based his assumption that IE *-*tt*- preceded by the accent became *-*st*- in Germanic.[25]

Similar objections may be made to the morphological correspondences he assumes: upon closer examination, the only convincing common innovation of Latin and Germanic is the use of the IE 'stative' in -*ē*- as the basis for a particular present formation.[26] The lexical correspondences, however, lead W. Porzig to some very interesting conclusions:

a. At the time of the close linguistic relationship between Germanic and Italic (meant as the proto-language from which both Latin and Osco-Umbrian developed), the speakers of the two languages must have been in direct contact; after the southward move of the Italic tribes, the 'Illyrians' came between them and the Germanic tribes

b. The period of close contact must have been from the oldest times of the IE community until the last third of the second millennium B.C.

c. As regards the position of Osco-Umbrian, wherever relevant forms occur in these languages, they correspond to Latin in 6 cases out of 7 when Latin and Germanic contrast with Celtic and in all 6 cases where Latin and Celtic contrast with Germanic, so that 'die idg. Ursprünge des Oskisch-Umbrischen und des Lateinischen nahe beisammen lagen'.[27]

d. Culturally, the Latino-Germanic isoglosses involve topographic terms and names of parts of the body, but especially a large technical vocabulary, including fishing, hunting, and mainly agriculture and law; characteristic is the absence of common innovations in the field of military terms. Compared with the Italo-Celtic vocabulary listed by Porzig where there are fewer technical terms and no law terms at all, the Latino-Germanic correspondences seem to point to a higher level of culture.

As for the Celto-Germanic correspondences, Porzig carefully selects those which are likely to belong to an older layer on account of the loss of /p/ in Celtic, of their vowel gradation or of the operation of the consonant shift in Germanic, and points out that they reflect 'Ausstrahlungen einer staatlich-gesellschaftlichen Kultur über Teile der Westindogermanen', in which Italic does not take part. For him, the center of irradiation of these cultural influences is the Celtic area, where 'die Ausbildung bestimmter Herrschaftsformen und einer eigentümlichen gesellschaftlichen Gliederung und die Entstehung des urkeltischen Sprachcharakters' are two aspects of one and the same historical process, which influenced the Germanic tribes at a later date without directly involving them.[28] Screening the Illyrian and Venetic material gathered by H. Krahe, he retains 6 isoglosses, which are supposed to reflect common innovations of Germanic and Illyrian, not shared by Celtic and Italic, and accordingly younger than the dissolution of the West IE community, but older than the consonant shift.

In the case of Germanic and Balto-Slavic, the problem of the relative dating of the isoglosses seems to be crucial: common innovations would point to lasting neighborly relations stretching from the oldest IE period

to the beginning of the Iron Age, as shown by the corresponding words for 'mill', 'rye', and 'silver'. Where Slavic and Germanic contrast with Baltic, the latter usually appears either to be more conservative or to have innovated on its own; the exclusively Baltic-Germanic correspondence reflects very old situations. The study of the lexicological isoglosses of Germanic with the neighboring languages accordingly shows contrary to Hirt that Germanic had closer relations with Baltic and Slavic than with any other language outside the Western group, but Porzig still believes that the link between Germanic and Latin was closer.[29]

In *Sprache und Vorzeit* (1954), the position of Germanic is again scrutinized by H. Krahe, whose main aim is to ascribe the linguistic unity of Old European hydronymy to the existence of a prehistoric linguistic community of IE origin covering the same territory. This community corresponds by and large to the civilization of the North-West, defined by Meillet on the basis of its vocabulary, though Krahe evidently adds Illyrian and Venetic to Germanic, Celtic, Italic, Baltic, and Slavic as languages which have developed from this 'Old European' stock. Otherwise, his views are rather conservative: the territory of the Old European community constituted a continuum in which the ancestors of the Italic and Celtic tribes progressively differentiated their speech within an originally common area; the ancestors of the Germanic tribes came closer to the Proto-Italic part of this area, and, hence, shared old common innovations both with Italic and Celtic or with Italic only. As the three groups developed as individual ethnic entities, the 'Italic' group moved out, and only then did Celtic and Germanic come into close direct contact. In the East, the ancestors of the Germanic tribes had presumably the ancestors of the Illyrian and Venetic as neighbors, so that they remained separated from the ancestors of Baltic and Slavic tribes until the Illyrians and Venetes moved out.[30] This would evidently imply that the oldest correspondences between Germanic and Balto-Slavic were originally shared by Illyrian, an assumption which is not backed up by the evidence.[31]

As a result, N. S. Čemodanov in his recent critical re-examination of the position of Germanic in the IE linguistic community,[32] though not following V. Georgiev in his overstatement of the importance of Balto-Slavic and Germanic correspondences,[33] tries to reconcile the views of Porzig with a better valuation of the Germano-Balto-Slavic isoglosses. Diachronically, in his opinion, there are two distinct stages in the relationship of both Slavic and Germanic with the neighboring IE dialects. Originally, Slavic was closer to Indo-Iranian, especially to Iranian, whereas Baltic was in close relation with Germanic. When Slavic broke away from Iranian, it became more closely linked with Baltic, whereas Germanic got involved in a closer relationship with Italic and perhaps also with Illyrian and Venetic. Only after the migration of the ancestors of the Italic tribes did Germanic and Celtic come into closer contact and develop their common features.[34]

This brief survey of the views expressed in major works on the position of Germanic among the IE languages clearly shows that phonological

and morphological correspondences hardly supply sufficient evidence to draw satisfactory conclusions regarding a closer relationship with neighboring dialects and that the bulk of the material referred to in the discussion is lexical, though common patterns of derivation may often be especially relevant. Leaving aside the problem of the relationship of Germanic with Illyrian and with Venetic, in which the limitation of the available material and the disputability of its interpretation, especially in the case of proper names, make any far-reaching conclusion hazardous,[35] and accepting provisionally the prevailing view that specifically Germano-Celtic isoglosses reflect younger contacts between the two linguistic groups than the lexical correspondences they share with Latin and Osco-Umbrian, the two main tasks confronting the student of the earliest relations of Germanic with the neighboring dialects are:

a. checking the validity of the Germano-Balto-Slavic and the Germano-Italic isoglosses, with special consideration of the correspondences of specifically Baltic, Slavic, and Osco-Umbrian lexical items with Germanic terms;

b. establishing the relative chronology of these sets of correspondences on the basis of linguistic and cultural criteria.

To illustrate the complexity of the problems that arise in connection with this double task, let us briefly examine a sampling of the evidence Čemodanov has retained as valid after carefully screening the material gathered and discussed by his predecessors. As regards Germano-Balto-Slavic lexical correspondences,[36] he lists 24 cases, where Porzig only mentioned 8 such isoglosses;[37] in addition, only 4 of the correspondences analyzed by Porzig are considered adequate by the Russian scholar, namely:

Goth. *gulþ*; ON *gull*; OE, OS, *gold*, OHG *golt*:
Lett. *zèlts*; E. Lit. *želtas*:
OCS *zlato*, Russ. *zóloto*, Cz. *zlato*, etc.

SHOWING THREE DIFFERENT GRADATIONAL FORMS of a theme I **ghel-t-*, the *o*-grade of Slavic also appearing possibly in the Venetic personal name *Goltanos*;

2. the word for 'mill' or 'millstone':

Goth. *qairnus*, ON *kvern*, OE *cweorn*, OHG *quirna*:
OPr. *girnoywis* (=*girnuwis*), Lit. *gìrnos*, Lett. *dzir̃nus*, *dzir̃navas*:
OCS *žrŭny*, Russ. *žërnov*, etc.

in which, however, ONLY GERMANIC shows a YOUNGER *-n-* derivation from a theme I **g^{w}er-H-*, contrasting with the enlarged theme II **g^{w}r-eH$_2$-w-* in Skt. *grā́van*, OIr. *brān*, whereas in Balto-Slavic **g^{w}ṛH-n-* may very well reflect the zero grade of **g^{w}r-eH$_2$-n-*, evidenced by Armenian *erkan*;

3. the numeral for '1,000':

Goth. *þūsundi*, ON *þūsund*, OE *ðūsend*, OHG *dūsunt*:
Lit. *túkstantis*, Lett. *tūkstuots*, OPR. *tūsimtons*:
OCS *tysęšta*, Russ. *týsjača*, etc.

for which M. Leumann has posited a prototype **tūs-ḱm̥tyom*, meaning 'Kraft-hundert'.[38]

4. the word for 'rye':
ON *rugr*, OE *ryge* <**rugi-*; OS *roggo*, OHG *rocko* < **rugan-*:
Lit. *rugỹs*, *rugiaĩ*, Lett. *rudzis* 'grain of rye· (plur.) rye':
OCS *rŭžĭ*, SCr. *raž*, Russ. *rož*', etc.

apparently a BORROWING FROM A NON-INDO-EUROPEAN LANGUAGE of the Caspian Sea area, which would have come to Germanic through Baltic; Thracian *brîza* 'rye' is probably from the same source.[39] Čemodanov could also have retained the verbal stem **waldh-* 'rule', similarly derived in the three languages by means of the *-dh-* 'determinative' from a theme II **wH₂-el-* 'be strong' (cf. the 'stative' *valē-* in Latin), used specifically to designate public authority or political supremacy. This would, however, have raised the question: CAN A PARALLEL DERIVATION WITH THE SAME SUFFIX FROM A DEFINITE IE ETYMON ALWAYS BE VALIDLY CONSIDERED AS A SIGNIFICANT ISOGLOSS BETWEEN TWO RELATED NEIGHBORING IE DIALECTS? Apparently the frequency of occurrence of *-dh-* affixation of themes II, particularly illustrated in Germanic noun- and verb formation by W. Lehmann,[40] has induced Čemodanov to avoid using its parallel occurence in Germanic, Baltic, and Slavic as a criterion of common development. On the contrary, when suffixes with low functional load appear in corresponding forms in the languages under consideration, he does not hesitate to refer to them as evidence for closer relationship, e.g. in the case of parallel **-twā-* derivation in Goth. *saliþwos* (plur.) 'hospitality', and CS *selitva* 'dwelling', though he only uses this morphological argument to give more weight to the occurrence of the root **sel-* in Germanic, Baltic, and Slavic in words for dwelling places: ON *salr* 'room, house', OHG *sal* 'dwelling': Lit. *salà* 'village': OCS *selo* 'village'.[41] Sometimes, however, Čemodanov's resorting to the clue of corresponding derivation rests on a rather subjective choice, as in the case of ON *kalla*, whose listing among the Germano-Baltic-Slavic isoglosses plainly indicates his preference of the reconstruction of a Germanic **kalzōn*, corresponding to Lit. *galsas* 'echo' and OCS *glasŭ* 'voice', over the reconstruction **kalnōn*, which a comparison with MIr. *gall* (<**galno-*), 'glory' would require, but herewith, he disregards the obvious possibility of an 'expressive gemination'.[42]

In the case of the word for 'snow', Porzig considered the contrast between the old athematic formation with zero grade stem **snigʷh-s*, appearing in Latin (*nix*), Greek (*níphα*), and Celtic (OIr. [plur.] *snechti*), and the younger thematic **snoygʷhos*, occurring in Germanic (Goth. *snaiws*, OE *snāw*), Baltic (OPr. *snaygis*, Lit. dial. *įsnaigas*), and Slavic (OCS *sněgŭ*) as quite significant for the dialectal division of Western Indo-European. Čemodanov is fully aware of the weakness of this argument in view of the Indo-Iranian forms pointing to **snoygʷho-*; e.g. Prākr. *siṇeha-* 'snow', Šugni, (Pamir dial.) *žəniǰ* (<**snaiga-*).

More complicated is the case of Goth. *hails* 'whole, healthy', ON *heill*, OE *hāl*, OHG *heil*: OCS *cělŭ* 'whole, healthy', Russ. *čel*, *čelyi*: OPr. *kailūstiskan* 'health' (from **kailūstas* 'healthy', *-st-* derivation from **kailus*),[43] where Porzig stresses the semantic correspondence, also pointing to the

use of the same words in forms of salutation in Germanic, Baltic, and Slavic. After W. Baetke's careful analysis of the use and meaning of the Germanic adjective,[44] Porzig is undoubtedly right in assuming that the word originally belonged to the IE religious vocabulary, where it applies to a benefic vitality emanating from the gods. This is clearly evidenced by the semantic correspondence between the Celtic cognates in W. *coel* 'omen', OW (plur.) *coil-ou.* auspiciis, OBret. *coel.* haruspicem, and Germanic verbs and phrases like OHG *heilisōn* 'observe presages', or ON *leita heilla* 'look for benefic signs', for, as Baetke emphasized it: 'hinter der Befragung des Gotteswillens [steht] die Heilserwartung . . . Das Opferorakel ist ein Mittel zur Heilsgewinnung . . . *heill* ist also nicht 'Glück' im profanen oder magischen Sinne, sondern eigentlich das kultisch vermittelte Heil'.[45] The concept 'sanus, integer' is accordingly derived secondarily from that of 'godly given well-being'[46] and the semantic correspondence between OCS *cěl"* and Goth. *hails* is merely due to parallel, but presumably independent development. Čemodanov was accordingly right in not giving this isogloss any weight for assuming closer a relationship between Germanic and Balto-Slavic. He might, however, have used the same restraint in the cases of:

(a) ON *ljóðr*, OHG *liut* 'nation', OE *leode*, OHG *liuti* 'people':
Lit. *liáudis* 'nation', Lett. *ļàudis* 'people, nation':
OCS *ljudŭ* 'nation', Russ. *ljud(i)*;

(b) Goth. *blinds*, ON *blindr*, OE *blind*, OHG *blint* 'blind':
Lit. *blandýti* 'wander, grow dark, lower one's eyes', Lett. *blenst* 'see indistinctly':
OCS *blędǫ* 'I wander'.

The second case can easily be discarded as unwarranted evidence on the basis of semantics, since the concept of 'blindness' is central in Germanic, which accordingly shows a specialization of the meaning, which Baltic and Slavic have not carried through in this *-dh-* enlargement of theme II **bhl-en-*; where the meaning 'blind' appears in Baltic, as in Lit. *blingỹs*, it is apparently based on borrowing from East-Prussian *blinger.*[47] As for the Germanic, Baltic, and Slavic reflexes of the enlarged IE theme II, **H₁l-ew-dh-* 'grow', Čemodanov fails to take into account the proper meaning of the terms with regard to the relevant social structure, as it appears from a closer examination of the language material and further consideration of the connotations of the concept of 'free' as expressed by Lat. *līber* and Gk. *eleútheros.* It is clear from Germanic law that the Latinized technical term *leudis* designates the 'freeman'; in Old High German, *liuti* originally meant 'heer- und dingberechtigte Mitglieder des Volksverbands', until the strict organization of the Franconian state reduced them to the status of 'subjects'; the word then became the mere designation of the 'ingenuum vulgus'. Though the Baltic and Slavic words have often been considered as borrowings from Germanic,[48] this assumption does not rest on sufficient grounds, as V. Kiparsky has shown,[49] and therefore the ORuss. translation of *laïkos*, *ljudinŭ* 'freeman', applying to the lower and middle

class as contrasted with the nobility and clergy, and OLit. *liáudis*, designating the 'common people' in Daukša's postil, provide nice parallels to the use of Germanic *leudis* als 'Vertreter des Volkes schlechthin, d.h. jener von Hörigen und Adligen abgehobenen Hauptmacht desselben, die entsprechend der jeweiligen soziologisch-politischen Entwicklung höher oder niedriger geachtet wird.'[50] Lat. *līber* and Gk. *eleútheros* similarly indicate membership in a social class, but focussed on the contrast with *servus* and *doûlos*, specifying their status as not being submitted to servitude. As E. Benveniste has convincingly shown,[51] the basic concept of Lat. *līber* and Gk. *eleútheros* is 'legitimate', lawful birth ensuring the citizen his full civil and political rights.

What apparently has happened is the common development of a legal meaning to the term originally designating the 'rising generation', whose first meaning has been preserved in Latin *līberī*. As Scheller suggests, the use of the term must first have been expanded to designate the 'erwachsenen echtbürtigen Angehörigen des Volkes . . . insofern diese ihrerseits sich als echtbürtigen Nachwuchs ihrer Vorfahren bezeichnen'.[52] This is further confirmed by the social function of the god of growth and vegetation *Liber* as protector of the popular community.

The last case of Germano-Balto-Slavic lexical correspondence, quoted by Porzig, namely: the word for 'fist':OE *fȳst*, OHG *fūst*:OCS *pęst'*:Lit. *kùm(p)stė*, is actually only a Germano-Slavic isogloss, as the Lithuanian word is rather related to Lit. *kumpas* 'bent, crooked' than developed through metathesis from **punkstė*.

Let us now briefly consider the other evidence adduced by Čemodanov. It is rather obvious that in one case, at least, he has given weight to structurally very different forms on the mere basis of meaning, namely in the word for 'lame' evidenced by:

Goth. *halts*, ON *haltr*, OE *healt*, OHG *halz*:
Lit. *klẽvas* ~ *šlẽvas*, *klývas*
Russ. dial. *koldýka*.

Apart from the fact that the Russian word can hardly be separated from *koltát'* and *kólča* 'cripple', in which the alternation *-d-* ~ *-t-* ~ *-č-* is presumably 'onomatopoeic,'[53] the Baltic words point to an enlarged theme II **kl-ey-w-* (with initial dialectal alternation **k-*:*ḱ-*), whereas Germanic and Slavic imply a theme I **kel-d-*, whose further connections are disputed.[54]

In the case of Germanic **warma-*, Čemodanov decides unilaterally in favor of the connection with Lit. *vìrti*, Lett. *vĩrt*, and OCS *vĭrěti* 'cook, boil', without even considering the possibility that IE **g^{w}h-* could be reflected by **w-* in Germanic, except before *-u-* and consonants,[55] which would make the obvious connection of Gmc. **warme-* with Lat. *formus* 'warm', Skt. *gharma-*, OPr. *gorme*, Lett. *gařme* 'heat' (:Gk. *thermós*, Arm. *jerm*, etc.) possible. The formal and semantic correspondence of **warma-* with the widespread IE word for 'heat' cannot, indeed, be readily dismissed unless more cogent evidence is adduced to show that IE *g^{w}ho-* should be reflected by **ga-* in Germanic.[56] Even so, Gmc. **warma-* would hardly be acceptable

as evidence for closer relation with Baltic and Slavic since these languages do not show a parallel formation in *-*mo*-, whereas semantically, except for OCS *varŭ* 'heat', the Baltic and Slavic words meaning 'bubble up, gush' beside 'cook, boil, seethe' are farther off from the Germanic adjective than the related Armenian and Hittite verbs: Arm. *vaṙem* 'light', *vaṙim* 'burn'; Hitt. *war-* 'burn', *warnu-* 'light'.

The other fourteen isoglosses quoted by Čemodanov comprise 6 sets of forms for which further IE cognates can be quoted and 8 which apparently remain quite isolated. The forms with IE cognates are:

a. ON *stolpi* 'post, pillar', MLG *stolpe* 'beam': Lett. *stulbs* 'post': OCS *stlŭba* 'stairs', Russ. *stolb* 'pillar, column', which apparently points to an enlarged theme II **st-el-b-*. The cooccurrence of Lit. *stulpas* 'post, stake, tower; idol', E. Latv. *stùlps* 'post' and OCS *stlŭpŭ* <**stŭlpŭ* 'column, tower' had led to the assumption of borrowing from Slavic by the Baltic dialects, Lit. *stulpas* showing remarkable correspondences in usage and meaning with ORuss. *stŭlpŭ* 'column, stake, tower; idol'. Anyhow, the Slavic forms imply the occurrence of the assumed theme II **st-el-* with a *-p-* determinative, for which there is no Germanic parallel, whereas only Germanic shows forms with the dental determinative *-d-* (OHG *stëlza*, ME *stilte* 'stilt'); besides, a form **stl̥-no-* 'stand' seems to be shared by Gmc. (OHG *stollo* 'support, post'; with 'ablaut' ON *stallr* 'stand, altar') and Gk. Thess. *stálla*, Dor. *stálā*, Att. *stélē* 'column'). Accordingly, in the light of the multiplicity of dialectally variously distributed enlarged forms of IE **st-el-*, the correspondence of ON *stolpi*, etc. with the Baltic and Slavic forms *-b-* becomes much less significant.

b. ON *lengja* 'strap, strip', OE *lōh* (<**lanha*) 'strap': Lith. *lankà* 'valley', *leñkti* 'to bend': OCS *lǫka* 'bend',[57] *lečí* 'rope'—apparently pointing to a theme II **len-k-* 'bend', whose occurrence is restricted to the three language groups, but paralleled by a theme II **len-g-*, appearing in Baltic, e.g. Lit. *lénge* 'tiefer gelegenes Stück Land, eine Vertiefung bildende kleine Wiese', and Slavic, e.g. SCr. *lûg* 'grove, reedbank', with remarkable alternating forms like Lit. *lénkė* 'tiefer gelegenes Land, eine Vertiefung bildende kleine Wiese'. The derived verbs Lit. *langóti*, Lett. *līguôt* 'move to and fro' would suggest relationship with Skt. *ráṅgati* 'moves to and fro', but this appears to be a young word without IE background, which leaves the isolated Albanian *lëngor* 'flexible' as the only IE cognate of the group. It should also be mentioned that the semantic development of the Germanic words sets them clearly apart from the rest of the group, since the Slavic noun for 'rope' is apparently closely connected to the OCS verb *lęcati* 'set traps or snares'.

c. Goth. *dius*, ON *dȳr*, OE *dēor*, OHG *tior* 'animal': Lit. *dvėsti* 'breathe one's last', *dvasià* 'spirit, breath', Lett. *dvaša* 'breathing, vapor': OCS *dyšǫ*, *dyxati* 'breathe', *duša*, Russ. *dušá* 'soul' reflecting the alternating themes I **dhew-s-*: II **dhw-es-* to the root **dhew-*, also appearing, e.g. in Gk. *thúō* 'storm, rage'. However, only Germanic shows the specialization of meaning from *anima* to *animal*.[58]

d. Goth. *wōpjan*, ON *ōpa* 'shout', OE *wēpan* 'weep', OHG *wuoffan* 'complain': Lett. *vābît* 'call to court': ORuss. *vabiti* 'lure, call (a bird)' —apparently pointing to a theme I *$*weH_2$-b-*, gut the Lettish verb is presumably borrowed directly from Byelorussian *vabit'* or Polish *wabić* or through Lit. *võbyti* 'lure'.[59] Quite a few parallel themes appear with various determinatives; e.g. *$*weH_2$-g-* (Lat. *vāgīre* 'wail, cry', Lit. *vógrauti* 'whine, prattle'); *weH_2-gh-* (Gk. *ēkhḗ* 'sound, noise'; with *s-* mobile: Goth. *gaswōgjan* 'sigh', ON *sōgr* 'noise'; Lit. *svagėti* 'resound'); etc. Though it is theoretically possible to reconstruct a root with a final laryngeal, the question remains open whether these forms are not basically onomatopoeic. In any case, while the basic Slavic term seems to be specialized to the terminology of hunting, no such specific meaning appears in Germanic.

e. ON *blīkja*, OE *blīcan*, OHG *blīhhan* 'shine': Lit. *blỹkšti* 'grow pale', *blizgėti* 'shine', Lett. *blaiskums* 'spot' : OCS *bliskati* 'shine'. All of them point to an enlarged theme II *$*bhl-ey-ǵ$-* from the root *$*bhel-$* 'shining, white'. The Baltic and Slavic are based on a *-sk-*present derived from this theme; however, Baltic *$*blīšk-$* seems to imply the coexistence of an original enlarged theme *$*bhl-eH_1-y$.

f. Goth. *dails*, OE *dǣl*, OHG *teil*; Goth. *dailjan*, ON *deila*, OHG *teilan*: Lith. *dalýti*, Lett. *dalīt* 'divide': OCS *dělъ* 'part', *děliti*.

The comparison is utterly improbable for the following reasons.

1. Lit. *dalýti*, Lett. *dalīt* belong together with Lit. *dalìs* 'part, share, fate', Lett. *daļa* 'part' from the same root *$*del-$* 'split, chop off' as OCS *dolja*, *dolъ* 'part, fate', from which technical terms like Lat. *dolāre* 'square (timber)' are derived;

2. The obvious IE connections of OCS *dělъ*, *děliti*, to which *dailýti* corresponds in Lithuanian, are Skt. *dáyate* 'divides', Gk. *daíesthai* 'divide', etc., which point to a root with initial *$*d-$*, whereas the Germanic forms imply *$*dh-$*. Consequently, unless this outside relationship of the Balto-Slavic forms is given up, they cannot be linked with the Germanic words, in spite of the parallel *-l-* determinative.

Though their structure is apparently IE, the remaining Germano-Balto-Slavic isoglosses listed by Čemodanov have no evident cognates outside these languages:

a. OE *delfan* 'dig', OHG *bitelban* 'bury': Lit. *dálba*, *délba* 'crowbar': Russ. *dolbīt'* 'chisel, chop'—all of them reflecting a theme I *$*dhel-bh-$*.

b. ON *stōð* 'herd of mares', OE *stōð*, OHG *stuot* 'herd of horses': Lit. *stódas* 'herd of horses': OCS *stado*, Russ. *stado* 'herd'—apparently reflecting *$*steH_2$-dh-*; but semantically, a connection with *$st(h)ā- < *stH_2- \sim *steH_2-$* 'stand' is improbable.

c. ON *vax*, OE *weax*, OHG *wahs* 'wax': Lit. *vãškas*, Lett. *vasks*: OCS *voskъ*, Russ. *vósk*—all pointing to a thematic noun *$*wokso-$*; whether a connection with the IE theme II *$*H_2w-eg$* 'weave' (e.g. OIr. *figim* 'weave') with the same *-s-* determinative as in Lat. *vēlum* 'veil, curtain' *$< *wek-s-lo-m$* (cf. also *vexillum* 'standard') can be assumed, remains doubtful, in spite of the apparent German parallel *Wabe* : *weben*.

d. ON *mǫkkr* 'fog, cloud' <**makkuR*: Let. *mãkuônis* 'cloud': OCS *mokrŭ*, Russ. *mokry* 'wet, damp'—semantically distinct insofar as the Baltic and Slavic words point to a meaning 'bog, swamp', which has often led to assuming a connection with OIr. *mōin* 'swamp, peatbog', W. *mawn* 'peat',[60] which others prefer to link up with Lat. *mānāre* 'trickle, run, flow',[61] though J. Vendryes is apparently not satisfied with either of these explanations.[62] In any case, the ON word remains quite isolated in Germanic as well.

e. ON *pollr* 'pond', OE *pyll* 'pond', Du. *poel* 'pool, bog': Lit. *balà* 'marsh, bog': OCS *blato*, Russ. *boloto* 'bog, swamp'—hardly convincing as the dialectal terms show considerable divergences:

1. The *-t-* suffix appearing in Slavic does not occur in the corresponding Baltic word, but is apparently found in words of Illyrian origin, like Old Dalmatian *balta* 'marshy lake', Northern Italian *palta* 'swamp', as well as in Albanian *bal'tɛ* 'mud, swamp'.[63]
2. As W. Schulze has shown,[64] swamps are often named after the color in which they appear in the sun- or moonlight: in the case of OCS *blato* and Illyrian **balta*, this color is probably 'white' (cf. Rumanian *Baltă Albă* 'white swamp'),[65] so that the words correspond to Lit. *báltas* 'white' (: *bãlas* 'white', *bálti* 'become white, get pale') and can ultimately be connected to the IE root **bhel-* 'shining, white'.
3. The Germanic words show a vowel alternation, of which a trace might also be found in Russ. *balarúžina* 'puddle', if its first component is **bala-* <**bhōla*, but the initial **bh-* of the IE root implied by its connection with OCS *bělŭ* 'white' <**bhēlo-* (: **bhəlo-* in Gk. *phalós* 'white'), precludes any relation with the Germanic words with initial *p-*.

f. ON *lāgr* 'low', MHG *lāge* 'flat': OPr. *līse* 'crawls', Lith. *lėkštas*, Lett. *lęzns* 'flat': OCS *lězǫ*, *lěsti* 'crawl'—all of them apparently reflecting a theme I **leH₁-ǵh-* whose connection (often suggested, but with hesitation) with **legh-* 'lay down, lie' would imply that this 'root' actually reflects a theme II **lH₁-egh-*.

g. ON *klǫkkr* 'soft, flexible' <**kleŋkwan-*, MHG *klinker* 'weak': Lit. *glėžnas* 'weak, flabby', Let. *glęzns* 'tender, soft': Bulg. *glézen*" 'spoiled'—apparently reflecting a theme II **gl-eǵ-* in Balto-Slavic; but this is hardly consistent with the nasalization in Germanic, which implies an enlarged theme II **gl-en-g-*.

h. ON *rǫ̈task* 'become merry', OE *rōt* 'friendly, merry': Lit. *rõds* 'willing': ORuss. *radŭ* 'libens', SCr. *rȁd*, Russ. *rad* 'glad' —hardly reflecting a theme I **reH₁-d-*, since the Lithuanian word is borrowed from Byelorussian or Polish *rad* and medieval Greek forms like *Ardágastos* (name of Slavonic chief), corresponding to Slavic *Radogostĭ* may well point to a Proto-Slavic stem **ārda-*.[66]

If we now try to summarize the results of this careful perusal of Čemodanov's Germano-Balto-Slavic isoglosses in order to point out their cultural implications, it appears that quite a few of them have to be discarded as noncogent for various reasons, namely because (1) upon closer analysis,

the quoted forms do not appear to be related, leaving the Germanic words isolated as in the case of Goth. *dails*, ON *mǫkkr*, Du. *poel*,[67] and ON *rǭ́task* and their dialectal cognates. In the case of Goth. *halts*, etc., the Baltic entries had to be dropped out of the set of corresponding words, so that, insofar as Russ. *koldýka* is not a Slavic dialectal innovation, we actually deal with a Germanic-Slavic isogloss, as in the case of the word for 'fist'; (2) the parallel affixation occurring in Germanic and Balto-Slavic is hardly significant enough in the context of the derivational pattern of the implied IE theme to warrant assuming a common development instead of the independent use of the same affix in the various dialects, e.g. in the case of **st-el-b-* (in ON *stolpi*, Lett. *stulbs* 'post'; Russ. *stolb* 'pillar'); a typical example where only a unilateral decision on the affixation could define the possible further connections of the Germanic word is ON *kalla*; (3) the meanings of the Germanic words indicate a definitely different semantic development from those of their Baltic and Slavic cognates; e.g. in the words for 'animal' (Goth. *dius*, etc.), 'blind' (Goth. *blinds*, etc.), and 'strap' (ON *lengja*, etc.); (4) the implied roots are obviously onomatopoeic and can hardly be projected into the proto-language, e.g. in the case of Got. *wōpjan* 'shout'; (5) the three languages have borrowed directly or indirectly from a common source, as in the case of the word for 'rye'; (6) the word under consideration actually covers a wider area than that of Germanic and Balto-Slavic, e.g. in the case of the root **sel-* in terms for 'dwelling' and of the adjective for 'healthy', where other North-Western dialects were involved, or in the case of the word originally designating the 'freeman', which appeared to be a technical term shared with Latin and Greek, reflecting a typical feature of West-Indo-European social organization, or again, if its initial *w-* reflects an IE **w-*, in the case of the adjective for 'warm', which would then be derived from an old IE verb for 'burn', preserved in the Northern marginal area of the IE territory. A special case is that of the word for 'mill(stone)' where Germanic and Baltic, and Slavic all three show an *-n-* derivation, but where Germanic innovates in the form of the stem, whereas Baltic and Slavic merely use the expected IE gradational form implied by the addition of the affixes *-n-* and *-w-* and actually show forms which are rather closer to Armenian.

After this rather severe screening, only 8 isoglosses out of Čemodanov's original 24 seem to deserve further consideration:

a. In the word for 'gold', in spite of the ablaut-alternation between Germanic, Baltic, and Slavic, the typical parallel use of the nominal suffix *-to-* to indicate the metal characterized by its yellow color can hardly be accidental.

b. The compound form for 'thousand' is undoubtedly a common innovation of the three languages, though various details in the phonetic development from the prototype postulated by Leumann remain to be cleared up.

c. In spite of the problem created by the gradational form **blīšk-* in Baltic, the restriction of the occurrence of the enlarged theme II **bhl-ey-ǵ-* to Germanic, Baltic, and Slavic with actual correspondence in semantic

usage makes it plausible to consider its formation as a common dialectal feature, but, again, it is always possible that the parallel use of the determinative *-g-* in the three languages is accidental; in this specific case, however, it should be pointed out that the other enlarged form of theme II **bhl-ey-*, namely *bhl-ey-k-*, appears only in Germanic (e.g. OE *blǣge* 'white bream') and Slavic (e.g. Russ. *blëknut'* 'become pale, fade'), and that a color-adjective in *-wo-* is derived directly from this theme in Baltic (Lit. *blývas* 'lilac, pale violet') and Germanic (**blīwa-*, applying to 'lead': OHG *blīo*, ON *blý*, etc.).

d. Of the five remaining correspondences, the words for 'wax' and for 'herd', especially of horses, are technical terms which may have been introduced regionally; in both cases, there is a remote possibility of deriving them from IE roots, but borrowing from local non-IE languages or introduction of a 'Wanderwort' is always possible with such words. The basic meaning of the group of OE *delfan* 'dig', Lit. *dálba* 'crowbar' and Russ. *dolbít'* 'chisel' remains too vague to make any precise assumption, but once again, we apparently are dealing with locally used technical terms. As for the last two cases, **lēgh-* 'low' or 'to crawl' and **gleǵ-* ~ **gleng-* 'weak, soft', they may represent relic-forms in marginal areas or borrowings from local non-IE languages. At any rate, the acceptable lexical evidence is hardly sufficient to draw any definite conclusions as to the level of civilization of the Germanic, Baltic, and Slavic tribes at the time of their contact. It merely confirms that they were familiar with gold, wax, horses, and implements to dig and chop off, but this points in no way to a new shared cultural development; perhaps the only clue about such progress is the creation of a common numeral for a multiple of 'hundred', since there are traces of usage of the Germanic form for '1,200' as well.

This rather negative result of the analysis of a very limited part of the evidence submitted in favor of the early relationship of Germanic with its eastern neighbors shows that the traditional method of listing isoglosses to draw far-reaching conclusions has to be thoroughly revised and that, for each correspondence, a careful discussion of the derivation, semantics, usage within the relevant language, dialectal position versus synonyms in other parts of the IE linguistic area, etymologies, possibilities of borrowing, etc., must precede any further elaboration on its validity as a clue for the dialectal position of the languages involved and for the level of civilization of their original speakers.

Notes

[1] Ueber die stellung der Italer innerhalb des indoeuropäischen stammes, *KZ* 7 (1858) 179–92.

[2] *Ibid.* 163–4, 166–7.

[3] *Die Deutschen und die Nachbarstämme* (Munich, 1837) 18: 'Dass das Griechische und Lateinische in ihren Bildungen unter sich selbst in näherer Verwandtschaft stehen, als mit andern Sprachen, ist bekannt. Dasselbe gilt von den nördlichen Gliedern dieser Familie, wie vom Deutschen und Slavischen. Nicht bloss Wurzeln, die

sich anderswo nicht wieder finden, haben beide Sprachen gemein; wie mit keiner andern zeigen sie unter sich Uebereinstimmung auch in der Umhüllung ihrer Wurzeln, zunächst in der Beugung.'

[4] Germanisch und slavisch, *KZ* 1 (1852) 141–4.

[5] 'Das ganze Gebiet der Declination ist . . . , abgesehen von der rein lautlichen Erscheinung des *m* = *bh*, für die Bestimmung des Verhältnisses der drei Familien (i.e. Slavic, Baltic, and Germanic) zu einander, wie es mir scheint, werthlos'. (A. Leskien, *Die Declination im Slavisch-litauischen und Germanischen* [Leipzig, 1876] 158).

[6] *Die Verwandtschaftsverhältnisse der indogermanischen Sprachen* (Weimar, 1872) 36ff.

[7] In H. Paul (ed.), *Grundriss der germanischen Philologie* 1 (Strasburg, 1891) 320.

[8] *Einleitung in die Geschichte der griechischen Sprache* (Göttingen, 1896) 109n2.

[9] 'Gegen eine nähere Verwandtschaft der beiden Sprachgruppen spricht zunächst die verschiedene Behandlung der Gutturale, die uralt und überaus einschneidend ist' (*loc. cit.* 292).

[10] *Urgermanische Grammatik* (Heidelberg, 1896) 11.

[11] A. Meillet, *op. cit.* 120.

[12] *I dialetti indoeuropei* (Naples, 1931) 175–6.

[13] *Studi sulla preistoria delle lingue indeuropee* (Rome, 1933) 68n2.

[14] *Op. cit.* 30: 'lo sviluppo linguistico ci fa presumere che uno dei primi atti nella soluzione dell'unità ie. sia proprio il formarsi d'una barriera politica e fors'anche religiosa fra Celti e Germani.'

[15] *Ibid.* 38.

[16] *Geolinguistica e Indeuropeo* (Rome, 1940) 204–8.

[17] Germanisch-lateinisch und Germanisch-oskisch-umbrisch, in *Germanen und Indogermanen, Festschrift für Herman Hirt* 2. 533–47, ed. H. Arntz (Heidelberg, 1936).

[18] Correspondances germano-osco-ombriennes, in *Revue des Etudes Indo-Européennes*, 1 (1938) 353–76.

[19] Cf., e.g., M. Bartoli, Die chronologische Stellung des Germanischen innerhalb der ario-europäischen Sprachen, *Neophilologus*, 18 (1933) 292–9; Il carattere arcaico dei linguaggi germanici, *Archivio Glottologico Italiano* 39 (1938) 52–68.

[20] *Op. cit.* 107.

[21] A. Martinet, Some Problems of Italic Consonantism, *Word*, 6 (1950) 27–8 (Also in *Economie des changements phonétiques* [Bern, 1955] 333–4); O. Szemerényi, The Development of the Indo-European Mediae Aspiratae in Latin and Italic, *Archivum Linguisticum* 4 (1952) 27–53; 5 (1953) 10–13.

[22] W. Lehmann, The Indo-European Resonants in Germanic, *Language* 31 (1955) 360.

[23] *Az idg. l̥, r̥ latin folytatása* (Budapest, 1941); cf. *KZ* 71 (1954) 200, fn. 1. If Szemerényi is right, IE [*l̥*] and [*r̥*] would, however, also be reflected by *ul* and *ur* in Illyrian, Albanian (S. Mann, *Language* 17 [1941], 12ff.) and maybe Hittite (O. Szemerényi, Beiträge zur hethitischen Grammatik, *Analecta Orientalia A. Csoma de Körös dicata* [Budapest, 1942/8] 14–6; cf. however, E. Benveniste, *Language* 29 [1953] 257–8).

[24] Cf. H. Pedersen, *Vergleichende Grammatik der keltischen Sprachen*. 1 (Göttingen, 1937) 47.

Welsh *ffrwst* 'haste', which is usually connected with Goth. *sprauto* 'quickly', may very well contain a *-st-* derivation from **sprut-* in Welsh *ffrwd* 'stream' (cf. J. Morris-Jones, *A Welsh Grammar–Historical and Comparative* [Oxford, 1913] 143, 156). This could also be the case with OBrit.-Lat. *ormesta*. miseria:Welsh *gormes* 'oppression' (the relation of the implied **med-stā* to OIr. *midiur* 'I judge' would be similar to that of **tep-stu-* in OIr. *tess* 'heat' to Lat. *tepēre* 'be warm'). As for Gaul. *Congeistlus* (personal name), OCorn. *guistel* 'hostage', W *gwystl* 'pledge', the Celtic form probably contains the complex suffix **-tlo-* versus **-(a)lo-* in Germanic, e.g. OHG *gīsal*, ON *gīsl* 'hostage', etc.

[25] Ueber einige germanische dentalverbindungen, *PBB* 7 (1880) 187–90. Cf., about these aberrant forms in *-st-*, G. van Langenhove, *Linguistische Studiën* 1 (Antwerp, s.a. [1936]) 58–63, 73–94.

[26] E. Polomé, Germanisch und Italisch im Lichte der deskriptiven Sprachbetrachtung (read at the IIIrd International Congress of Germanists, Amsterdam, Aug. 25, 1965; published in *Orbis* 15 [1966] 190–9). On the *-ē-*verbs in Germanic, cf. also my contribution, On the origin of Germanic Class III of weak Verbs, *Beiträge zur Indogermanistik und Keltologie* (Innsbruck, 1967) 83–92.

[27] W. Porzig, *op. cit.* 117.

[28] *Ibid.* 123.

[29] *Ibid.* 147.

[30] Similar views were also presented by E. Schwarz, Germanen, Italiker, Kelten, *Zeitschrift für Mundartforschung* 20 (1952) 193–206.

[31] W. Porzig, *op cit.* 143.

[32] *Mesto germanskix jazykov sredi drygix indoevropejskix jazykov, Sravnitel'naja grammatika germanskix jazykov* 1 (Moscow, 1962) 19–113.

[33] *Isledovanija po sravnitelno-istoričeskomu jazykoznaniju* (Moscow, 1958) 237: 'in comparison with all the other groups of Indo-European languages, Balto-Slavic is more closely connected with Germanic.'

[34] N. S. Čemodanov, *loc. cit.* 112–3.

[35] On Venetic and Germanic, cf. my contribution to MNĒMĒS KHARIN. *Gedenkschrift P. Kretschmer* 2 (Vienna, 1957) 86–98; on Illyrian and Germanic, cf., provisionally, my brief remarks in *Latomus* 20 (1961), 143–5, and The Position of Ilyrian and Venetic, in H. Birnbaum and J. Puhvel (eds.), *Ancient Indo-European Dialects* (Berkeley-Los Angeles, 1966) 63-4.

[36] *Loc. cit.* 81–5.

[37] *Op. cit.* 140–3.

[38] *IF* 58 (1941) 126–8 (= *Kleine Schriften* [Zürich-Stuttgart, 1959] 357–8).

[39] This seems, at any rate, more plausible than to posit an IE prototype **wrughyo-* on the basis of the Thracian form, the initial *w-* being lost elsewhere.

[40] *Language* 18 (1942) 125–32.

[41] The occurrence of ON *salr* in *Vǫluspá* st. 4 in the meaning 'ground, soil' (H. Gering, *Kommentar zu den Liedern der Edda* 1 [Halle, 1927] 7), as well as the use of OCS *selo* to translate both *agrós* and *skēnḗ* suggest the inclusion of Latin *solum* 'soil' (also 'pavement, foundation') into this group with the original meaning 'human settlement'. A. Ernout-A. Meillet, *Dictionnaire étymologique de la langue latine* (3rd ed., Paris, 1951) 1119–20.

[42] Cf. the numerous parallels in verbs meaning 'to speak' and 'to call' in A. Martinet, *La gémination consonantique d'origine expressive dans les langues germaniques* (Copenhagen-Paris, 1937) 172–3.

[43] E. Fraenkel, *Die baltischen Sprachen* (Heidelberg, 1950) 45, rather thinks of the derivation of an abstract noun in *-ūt-*, ascribing *-st-* to the phonetic development of **-tt-*, and he compares the Slavic *-ū-* stem in OCS *cěly* 'healing'.

[44] *Das Heilige im Germanischen* (Tübingen, 1942) esp. 57–79, 215–26.

[45] *Op. cit.* 66.

[46] This basic meaning would also be reflected by the Greek gloss *koilu*: *tò kalón* (Hesychius) if it is related to the group as a reflex of the 'enlarged' theme II **kH₂-oy-l-*(: **kH₂-éy-l-* in the other languages).

[47] Cf. E. Fraenkel, *Litauisches etymologisches Wörterbuch* (Heidelberg, 1955) 48.

[48] Cf., recently, A. Vaillant, *Grammaire comparée des langues slaves* 1 (Paris, 1950) 123.

[49] *Die gemeinslavischen Lehnwörter aus dem Germanischen* (Helsinki, 1934) 72–4.

[50] M. Scheller, *Vedisch priyá- und die Wortsippe frei, freien, Freund* (Göttingen, 1954) 37 (fn. 1 to p. 36).

[51] *Revue des Études Latines* 14 (1936) 53ff.

[52] *Op. cit.* 42.

[53] E. Berneker, *Slavisches etymologisches Wörterbuch* 1 (Heidelberg, 1924) 660.

[54] If the original meaning is 'with a broken limb', OIr. *coll* 'destruction, damage' (< **koldo-*) would obviously be cognate, the root being found in the Greek adjective *kólos* 'docked' (of lance); cf. J. Pokorny, *Indogermanisches etymologisches Wörterbuch* (Bern, 1951) 545–7. Most improbable is the connection with Lat. *claudus*, which is still proposed by J. de Vries, *Altnordisches etymologisches Wörterbuch* (Leyden, 1958) 206.

[55] E. Polomé, *Revue belge de philologie et d'histoire* 26 (1948) 559–68.

This extensive discussion of the etymological material has been overlooked by E. Diebold in his recent study Die Vertretung von idg. **g^{u}h-* im Germanischen, in *KZ* 81 (1067) 104–33, where some of the generally discarded nineteenth century etymologies assuming that initial IE **g^{w}h-* is reflected by *b-* in Germanic, are rather unconvincingly revived. Thus, Gmc. *brenna-* 'burn' is considered as a *-nu-* present to the root **g^{w}her-* 'heat, burn', but no explanation is supplied to account for the full grade form with initial *br-* instead of the zero grade with initial *g-* before *-u-* that would be expected, according to Diebold's rules, in the Gmc. reflex of a secondarily thematized IE **g^{w}hṛnwéti*, from the theme II with nasal infix **g^{w}hṛ-n-ew-* implied by Skt. *ghṛṇóti* 'shines, burns' (Dhātup.).

[56] Cf. the discussion of ON *gaman* 'joy', *gandr* 'magic wand' and *geþ* 'mind, conscience' in J. de Vries, *Altnordisches etymologisches Wörterbuch* (Leyden, 1957) 154, 155, 159.

[57] Čemodanov translates OCS *lǫka* 'izvilina'; in the OCS texts, the word actually corresponds to Gk. *dólos panourgía*, i.e. 'cunning, treachery; villanies' (cf. L. Sadnik and R. Aitzetmüller, *Handwörterbuch zu den altkirchenslavischen Texten* [Heidelberg-The Hague, 1955] 52: 'Arglist, Bosheit; Pl. Ränke'); in later Church Slavonic, it is also glossed by Gk. *kólpos* in the meanings 'gulf' or 'vale'. The basic idea of 'bend', curve' is also reflected by OCS *lǫkŭ* and Lit. *lañkas* 'bow'.

[58] The only parallel would be Lat. *bestia* 'beast' if it reflects **dhwestyā*, which remains extremely doubtful, on account of the initial *b-* (cf. Lat. *suffiō* 'smoke' : Gk. *thúō* 'offer by burning', parallel to **dhwor-* > Lat. *forēs*).

[59] K. Mühlenbach-J. Endzelin, *Lettisch-deutsches Wörterbuch* 4 (Riga, 1929–32) 490; E. Fraenkel, *Litauisches etymologisches Wörterbuch* (Heidelberg, 1963) 1270.

[60] Cf., recently, M. Vasmer, *Russisches etymologisches Wörterbuch* 2 (Heidelberg, 1954) 148.

[61] A. Walde-J. B. Hofmann, *Lateinisches etymologisches Wörterbuch* 2 (Heidelberg, 1940) 30–1; A. Ernout-A. Meillet, *Dictionnaire étymologique de la langue latine* (Paris, 1951) 684–5.

[62] *Lexique étymologique de l'islandais ancien* (Dublin-Paris, 1960) M-60.

[63] W. Porzig, *op. cit.* 175, considers the group as 'die einzige Uebereinstimmung des Albanischen mit dem Slavischen unter Ausschluss des Baltischen, das hier den älteren Typus ohne *-to-* bewahrt hat.'

[64] *Kleine Schriften* (Göttingen, 1934) 111–21.

[65] E. Fraenkel, Die baltischen Sprachen (Heidelberg, 1950) 19–20.

[66] M. Vasmer, *Russisches etymologisches Wörterbuch* 2 (Heidelberg, 1955) 481.

[67] H. Kuhn, Anlautend *p-* im Germanischen, *Zeitschrift für Mundartforschung* 28 (1961) 9, considers Gmc. **-pōl-* with its ablaut variants as borrowed from an IE dialect with a pattern of gradation different from Germanic.

Tocharian: Indo-European and Non-Indo-European Relationships

George S. Lane

Almost from the very beginning of Tocharian studies, the question of the relationship between these two newly discovered Indo-European dialects of Central Asia and the other branches of Indo-European came under vigorous discussion. In fact, in their famous first publication and analysis of a Tocharian text, 'Tocharisch, die Sprache der Indoskythen,'[1] E. Sieg and W. Siegling themselves could not refrain from commenting on the 'viele zweifellose Übereinstimmungen mit der europäischen Gruppe der indogermanischen Sprachen.' Likewise, R. Pischel in his 'Nachwort' at the end of the article by Sieg and Siegling called attention to similarities of phonetic development between Tocharian and Germanic, Armenian, and some of the modern Indian dialects, especially the so-called Paiśācī. He referred particularly to the unvoicing of original voiced stops in all these languages, but he refused, correctly enough, to lay much stress on the Tocharian-Indian parallels. He did, however, emphasize one point: the undoubted influence of other languages on the development of the Tocharian declension.

Strange as it might seem, the earliest published discussion of Tocharian outside Germany which I can find was that of Maurice Bloomfield in a paper before The Johns Hopkins Philological Club on October 23, 1908.[2] Bloomfield here makes the erroneous comparison of the comitative suffix A *-aśśäl* with the Mittani forms of the Aryan gods *mi-ti-ra-aš-ši-il* and *U-ru-w-na-aš-ši-el*, and of the pl. suffix A *-añ* with Mittani *na-aš-at-ti-ia-an-na* (= Skt. Nāsatyās). The former comparison was made later also by E. Lewy when there was no longer an excuse for it (cf. infra).

That a hasty examination of this first publication of Sieg and Siegling would impress the scholarly world with the Western character of the language is not surprising, especially when the word for '100' was revealed as A *känt*, B *kante*. Indeed Tocharian was at once hailed as a 'centum' language.

The first complete comparative study of the language based on the materials furnished by Sieg and Siegling was that of Emil Smith 'Tocharisch, die neuentdeckte indogermanische Sprache Mittelasiens.'[3] In this essay, Smith goes several steps beyond his predecessors, some of which had to be retracted. On the basis of the presumed development of IE k^w to p (e.g.

lip- 'be left': Gk. *leípō*, Lat. *linquo*, etc.) and of some fifty proposed etymologies, he suggests (p. 43) a particular affinity for Greek with slightly less close ties to Armenian, Thracian, and Phrygian.

The possible development labiovelars to labials in Tocharian is to remain with us for a long time. It is mentioned again by Feist in his important work *Kultur, Ausbreitung und Herkunft der Indogermanen* (pp. 430, 445)[4] but with some reservations. However, it was already rejected by Meillet in his survey 'Le tokharien' in *Indogermanisches Jahrbuch* I, 15.[5] Meillet's conclusion as to the position of Tocharian in relationship to the other IE dialects is as follows: 'On ne se trompera sans doute pas beaucoup en attribuant au tokharien une place intermédiaire entre l'italo-celtique d'une part, le slave et l'arménien de l'autre. Mais les faits dont on dispose ne permettent aucune conclusion certaine.'

A later article by Meillet, together with Lévi, on the Tocharian verb published in the *Mémoires de la Société de Linguistique de Paris* 18 (1913) 1ff.) was of great importance, for, for the first time, we are furnished a fairly clear picture of the verbal endings, particularly of the medio-passive in *-r*, in dialect B. As was to be expected, the striking (outward) similarity to the deponent-passive in Italic and Celtic was bound to evoke interest. Meillet himself remarks (p. 13): 'On ne concluera pas nécessairement de là que le tokharien est particulièrement proche de l'italo-celtique, mais seulement que la caractéristique *-r* existait déjà en indo-européen et n'est pas une innovation italo-celtique.'

The first longer discussion of this question was by Vendryes in the *Revue celtique* 34 (1913) 129–42. The author's conclusions were, like those of Meillet, modest and cautious. Neither the deponent-passive in *-r* nor the *ā*-subjunctive (which is here considered carefully for the first time), he concludes, justifies the assumption of a new dialect grouping.

Not so Jarl Charpentier. In a detailed study 'Die verbalen *r*-Endungen der indogermanischen Sprachen'[6] he concluded (p. 42) with regard to the forms in *-r* 'dass man absolut nicht umhin kann, sie alle in näheren Zusammenhang mit einander zu rücken.' And Charpentier later, in an article entitled 'Die ethnographische Stellung der Tocharer', (*ZDMG* 71, 1917, 347–88) uses the endings in *-r* as the chief basis for his conclusion 'dass die Yüe-chï, die späteren *tókharoi* oder Tu-huo-lo, ein nach Zentralasien verschlagener keltischer Stamm waren, die später in Baktrien ein eigenes Reich gründeten, und denen die Sprache, die wir "Tocharisch" nennen, mit recht zugeschrieben wird' (p. 387).

Charpentier's enthusiastic, but badly founded Celtic theory, is followed two years later by Julius Pokorny's important lecture 'Die Tocharer und die Wanderung der Kelten in Asien' before the Forschungs-Institut für Osten und Orient in Vienna.[7] Pokorny, as one of the best-informed Celtic scholars of the world, is able to refute completely the arguments advanced by Charpentier. His own conclusion is that, on the basis of linguistic evidence and historical possibilities 'wir werden die Tocharer als thracophrygische Kimmerier ansehen dürfen,' and that 'Die Übereinstimmungen mit

dem Armenischen erklären sich daraus, dass beide Sprachen dem thraco-phrygischen Sprachstamme angehören und beide von den Kaukasus-Sprachen beeinflusst worden waren.'[8]

Possibly the earliest important discussion of the place of Tocharian among the IE dialects which is based upon enough published materials so that valid conclusions might be reached, is that by E. Hermann in his review of the *Tocharische Sprachreste*[9] in *KZ* 50 (1922) 296–314. The author attempts to correct and make more precise Pokorny's earlier attempt to identify the language as that of the 'Thraco-Phrygian Cimmerians.' According to Hermann, it is most closely allied to Phrygian, with the latter occupying a middle position between Tocharian and Armenian.

Hermann also undertakes for the first time now a discussion of probable outside, i.e. non-Indo-European influences on Tocharian—influences which might account for some of the peculiarities of Tocharian structure. He is sceptical of Pokorny's suggestion of influence by the languages of the Caucasus, and would rather seek the source of some features of Tocharian grammar in Tibetan. He mentions in particular the distinction between masculine and feminine forms for the pronoun 'I' in Tocharian and the similarity between the formation of the declension in Tibetan and Tocharian. He fails, however, to find any lexical influence (p. 311). To my knowledge, any evidence of borrowing by Tocharian from Tibetan still remains to be found (cf. below).

In his monograph *Indogermanen und Germanen*,[10] Sigmund Feist turns again to the Tocharian problem and this time in some detail. However, his conclusions are largely negative as regards the relationship of the language to the other branches of the family: 'Es ergibt sich also aus den vorangehenden angeführten Tatsachen und den theoretischen Erwägungen, dass das Tocharervolk oder richtiger seine indogermanische Oberschicht, die Arsi (Asii, Asiani) nicht aus Europa gekommen sein kann, und noch weniger als Kelten oder als Verwandte der Armenier anzusehen ist.' Feist also blames the 'Zerrüttung' of the Indo-European characteristics in Tocharian on the contacts with and indeed absorption of non-Indo-European elements by the original speakers of the language during their long migrations through Asia. And indeed, in passing, he comments that the poverty in stops is to be observed in Finno-Ugric.[11] Likewise, Wilhelm Schulze remarks that the formation of the case system of Tocharian along with the *u*-coloring of vowels preceding or following original labiovelars is what one might expect 'von einer später unter fremden Einfluss geratenen centum-Sprache . . . '[12]

With the continued decipherment of the Hittite language and the grammatical and etymological exploitation of its texts, the relationship of the two newly discovered Indo-European languages was found to be discussed. E. Lewy, in *KZ* 45 (1913) 45, called attention as Bloomfield had done five years earlier to the resemblance between A-*aśśäl* and the Mittani *mi-it-ra-aš-ši-il*, but apparently was under the impression that the latter form was Hittite.

The first major contribution devoted particularly to IE dialectology after the decipherment of both Tocharian and Hittite was Holger Pedersen's monograph *Le groupement des dialectes indo-européens* published in 1925.[13] Pedersen here takes no clear position on the matter, but rather simply points out the agreements between the two languages. On the other hand, W. Petersen in an article in *Language* 9 (1933) 12–34, maintained a close relationship between them, so close in fact that they were to be considered as having both separated from the parent speech earlier than and independent of the rest of the IE family.

In 1941, Pedersen returns to the question in his *Tocharisch vom Gesichtspunkt der indoeuropäischen Sprachvergleichung*[14] and vigorously argues against Petersen's view, not only from the point of view of the validity of his evidence, but also from that of his methodology.

The most recent and the most important discussion of our problem is that of Émile Benveniste 'Tokharien et indo-européen' in the *Hirt Festschrift*.[15] Benveniste covers with thoroughness the various isoglosses which Tocharian shares with the different branches of Indo-European, and, in particular, pays careful attention to the community of vocabulary with the western languages, with Hittite, and with Slavic, Baltic, Thraco-Phrygian, and Greek (pp. 234ff.). His conclusion is that 'le tokharien est un membre ancien d'un groupe préhistorique (auquel appartenait peut-être aussi le hittite) qui confinait d'une part au baltique et au slave, de l'autre part au grec, à l'arménien et au thraco-phrygien' (p. 237 bottom).

Benveniste goes further and attempts to localize the home of these 'Proto-Tocharians' geographically. His conclusion is for some region of the steppes in southeastern Russia. They were thus bordered on the west and south by the Thraco-Phrygians, the Armenians and the Greeks; on the west and north by the Germans and the Balto-Slavs, in close proximity to the Finno-Ugrians on the east and north. In the latter connection, Benveniste calls attention to the resemblance of the word for 'gold', A *wäs*, B *yasa* to the Finnish word *vaski* 'copper' and the Arm. *oski* 'gold'. The Tocharian words are of course usually derived from **wes-* beside **aws-* in Lat. *aurum*, Old Lith. *ausas*, etc.[16] The author considers also important in connection with the geographical location of the Proto-Tocharian the word for 'bee', *A kroṅśe*, B *kroṅkśe*, *kroṅśe* (which, however, according to the usually accepted etymology originally referred to the 'hornet', cf. OHG *hornuz*, OE *hyrnet*, Lat. *crābrō*, etc.) indicating that they belonged originally, as the Slavs and Germans, to an agricultural population who practiced apiculture. Likewise, the word for 'salt', *A sāle*, B *salyiye* would point to the proximity of the immense deposits of salt about the mouth of the Dniepr.

However, perhaps the one piece of vocabulary to which the greatest importance has been attached is the word for 'fish', *laks* in dialect B, which recalls so forcibly the name of the 'salmon' in Germanic, OHG *lahs*, Lith. *lašišà*, *lãšis*, OPruss. *lasasso*, Russ. *losos'*, etc. Almost every discussion of the relation of the Tocharians to the other branches of IE regards this word

as proof positive that the 'home' of the Tocharians was situated far enough to the north for the Proto-Tocharians to have been acquainted with the Salmon which is only found in the rivers flowing into the North Sea and the Baltic, since those rivers tributary to the Caspian and the Black seas do not know the salmon. I shall have occasion to return to this argument later.

In this paper, insofar as I am concerned with it, I shall accept rather the southeastern European theory, since one must accept as more valid the arguments on one side of a question than those on the other, especially when one feels oneself to be not competent to make independent decisions in the matter at hand.

My own views as to the original dialectal position of Tocharian with relation to the other IE dialects are largely those of Professor Benveniste presented in the study cited above. 'Then,' you might say, 'why are you presenting a paper on the announced subject at all?' You will note, however, I said 'the ORIGINAL dialectal position.' What I am interested in here is in the LATER contacts which the bearers of the Tocharian language had with other linguistic groups, Indo-European and non-Indo-European.

Possibly the original home of the Indo-Europeans is not as important, but the original home and later movements of the Proto-Tocharians are, and certainly the area proposed by Professor Benveniste makes these a lot simpler to account for.

The four branches of Indo-European to which the relations of Tocharian should be carefully reexamined are, I believe, Italic, Celtic, Germanic, and Balto-Slavic.

With regard to the first two, Italic and Celtic, there are two formations which they share with Tocharian which should be reexamined, namely, the medio-passive in *-r* and the subjunctive in *-ā*. The former of these has of course come in for its share of discussion in the past. Possibly the reason for the hesitation to revive it is the exaggerated significance lent it by Charpentier when he argued that Tocharian was a Celtic language largely on the basis of that single criterion.[17] But the fact that a strikingly parallel development in Tocharian, Italic, and Celtic of the use of this element *r*, which, according to the Hittite evidence, was once an independent and apparently optional morpheme must have some significance. I hardly believe we can shrug off this parallelism simply by pointing to the presence of the similar (medio-passive) *r*-forms in Phrygian *addaketor* and *abberetor* 'er fügt zu'. It can well be that if we had sufficient evidence, we would have to include Phrygian as one of the languages which shares generously in this particular isogloss. As for Armenian, I believe the analysis of the *r*-forms is too obscure to be of much use in this connection. If indeed the third singular imperfect *berēr* does reflect an original **bheretor* and is therefore to be equated to Phrygian *(ab)beretor* then we have merely an early isogloss shared by Armenian also. As regards the other (active) *r*-forms in Armenian, i.e. the 2nd sg. act. impf. *bereir* and aor. *berer*, pass. *berar*, along with the prohibitive *mi berer*, they are generally conceded as owing their *-r* to an attached particle related to the Gk. *rha* or Lith. *ĩr*.[18]

Of importance here, it seems to me, is the fact that the *r*-forms of the medio-passive are limited to the present (indicative and subjunctive) as is the Hittite *-ri*, but as opposed to the latter, the *-r* has been extended to all persons. In Hittite, *-ri* is a characteristic of the entire singular and the third plural, and is found also in the second plural and *-ru* is obligatory in the imperative 1st and 3rd sg. and 3rd pl.[19]

If I judge the situation rightly in Italic and Celtic, the extension of *-r* to nonpresent tenses must be late. Note the fact that there is no simple deponent perfect passive in Latin, and in the Italic dialects the periphrastic forms of the perfect passive are more frequent than the *r*-forms.[20] Even in Irish where we find the most widespread occurrence of *r*-endings in both the deponent and the passive, they have not been extended in the former to certain past tenses, namely, the imperfect indicative, the preterit subjunctive and the so-called preterit future nor to the preterit passive.[21] The 2nd pl. in both Latin and Irish rejects the *-r*, but the forms are otherwise not comparable. Also in the 2nd sg. the addition of *r-* in Irish seems late and the form in no wise corresponds to the Latin which is rather derived from the secondary middle *-so* > *-re*, later (with added *-s*) *-eris*.[22]

A possible interpretation of the evidence here presented as regards the relations of Tocharian and Italic and Celtic would be, I think, that, while in no way indicative of any original close dialectal unity between them, it does point to rather a long period of close contact after the separation of Tocharian from the nearest of kin, Thraco-Phrygian (and perhaps Armenian). During this period of close contact, the *r*-formation had clearly become a fixed characteristic of the third persons, both singular and plural, and possibly of the first persons, but not of the second person singular or plural. It had not extended beyond the present tense. That is, it had become fixed in those forms where in Hittite it remained optional. The further extension to the second persons is within Tocharian itself and to other tenses is within Italic and Celtic, but independent in both and effective to any extent only in the latter.[23]

I turn now to an evaluation of the *ā-* subjunctive.[24]

Some years ago, in an article entitled 'Tocharian Evidence and the Trubetzkoy-Benveniste Hypothesis',[25] I believe I succeeded in showing that so far as Tocharian was concerned, the evidence of the *ā*-preterit and the *ā*-subjunctive showed that the preterit value of this formation was the earlier. This was not of course a new idea with me. That the *ā*-subjunctive of Italic and Celtic had developed out of an 'injunctive' use of the *ā*-preterit was also the opinion of Brugmann[26] and Pedersen[27] even without reference to Tocharian. The peculiarity and importance of Tocharian in settling the question lies in the fact that the *ā*-preterit is an extremely abundant formation alongside the *ā*-subjunctive. The situation is not as in Italic and Celtic where the *ā*-preterit has, except for a few relics of disputed antiquity, been entirely eliminated and the *ā*-formation preserved only as a present (indicative and subjunctive) stem. The same *ā*-preterit is, to be sure, found in Armenian and therefore belongs to the old set of isoglosses connecting the

latter language with those under discussion, but its use has been greatly restricted.[28]

On the other hand, the *ā*-formation is found particularly in Baltic and Slavic, in the former both as a present and as a preterit formation and in the latter only as a preterit (except for the one verb OCS. *imamĭ* 'I have').[29]

Tocharian therefore stands out among the Indo-European languages as preserving the *ā*-formation in its original value as a preterit and in its secondary use as a subjunctive. This situation is due again to its position among its sister dialects after its separation from its more original closer relatives. As in Italic and Celtic, the original use as an injunctive led (with 'primary' endings) to a predominantly modal use, but a few verbs even with such endings remained as indicatives. On the other hand, under the influence of Slavic the preterit value was retained and even expanded.

It was no doubt with the Slavic speakers that the Tocharians had their last Indo-European contacts before the very late influence of the Iranians and Indians. This latter, I might mention in passing, was very superficial and of a cultural and literary nature resulting only in vocabulary borrowing, not, as far as I can see, in any even incipient influence on the structure of the language. One might go so far as to say that the decided inclination toward the use of possessive adjectives in place of genitives might be blamed on Slavic contacts.

As far as the contacts of Tocharian with Germanic are concerned, I see none of particular significance which would indicate a community of development such as I have indicated in the case of Italic, Celtic, and Balto-Slavic. The fusion of aorist and perfect into a preterit, which of course occurs also in Italic, Celtic, and Slavic, I see to be of little importance, and long vowel preterits are probably a matter of independent parallel development wherever they occur, in Tocharian or elsewhere.

The fact that Tocharian is a so-called 'centum' language need hardly be alluded to again. I myself do not consider the presence or absence of fusion of the plain velars with palatals on the one hand or with the labiovelars on the other to be of fundamental importance from the dialect point of view. And, in fact, it is the evidence of Tocharian and of Hittite which has assisted in depriving such criteria of much if any dialectal significance. I refer here especially to one of Antoine Meillet's last articles 'Les gutturales et le tokharien.'[30]

Let us turn now to a consideration of the possible influences of other entirely unrelated languages on Tocharian after its complete separation from contacts with other Indo-European languages.

Here, the first possibility that comes to the mind is of course Finno-Ugric, and indeed such influence has already been suggested, first perhaps by Feist[31] who calls particular attention to the single series of voiced stops, *p*, *t*, *k*. Such poverty in the system of oral stops in Proto-Finno-Urgic is, I believe, generally assumed.[32]

It might be worthy of mention also that an extensive series of affricates and sibilants are likewise assumed for Proto-Finno-Ugric. Might it not

therefore be just possible that this type of articulation could have had some influence also on Tocharian and would account for the extensive so-called palatalization, which seems still to a great extent to be entirely without rule?[33]

A recent serious attempt to explore the problem of non-Indo-European influences on Tocharian developments was undertaken by Wolfgang Krause.[34] Here the author was concerned in particular with morphological processes.

In this study, Krause surveys the possibility of such influences from Tibetan, Dravidian, the languages of the north Caucasus, Turkic, and Finno-Ugric. Influence from Tibetan, often suggested, is as the author says, to be rejected on known historical grounds, and the prehistory of the Dravidians is so unclear as to make any agreements in structure, no matter how striking they may be, purely conjectural. As for Turkic, the vernacular language we know for a fact to have been in closest contact with Tocharian during the historical period, so far no probably significant agreements between Uigur and Tocharian have been brought to light, though prehistoric contacts with older Turkish or other Altaic dialects can be assumed.[35]

It seems improbable to me that the languages of the Caucasus could have been of any long-enduring influence on Tocharian. In spite of Heine-Geldern's[36] attempts to show that the Tocharians were a part of an eastward migration from the Pontic steppes in the ninth and eighth centuries B.C., which followed a more southern route, I believe the linguistic evidence I have presented shows that they in all probability occupied a more northern area of European Russia at a considerably earlier time. I am inclined to agree with Denis Sinor's remark with regard to Heine-Geldern's study that it collects 'une bonne documentation archéologique pour jeter une lumière sur des évènements qu'à mon avis cette discipline ne peut éclaircir.'[37] The ancestors of the Tocharians may indeed have been Cimmerians as Heine-Geldern thinks, and as both Pokorny[38] and Charpentier[39] before him thought (the latter however made them into Celts!), but as long as we have no documents containing evidence on the language the Cimmerians spoke, we know nothing of their linguistic affinities.[40]

There remains then the question of the influence of Finno-Ugric on Proto-Tocharian, or, indeed, Pre-Tocharian might be better. Here it is, of course, the so-called agglutinative character of Tocharian declension that meets the eye, but this type of inflection is found in many other languages, in particular in the Altaic family, and so could be attributed to later more Asiatic influence. But there is a much more significant agreement between Finno-Ugric and Tocharian which Krause has most properly emphasized,[41] and that is the prevalent use of dvandva-type compounds like A *ñom-klu*, B *ñem-kälywe* 'fame', lit. 'name (and) report' (last part: A B *klaw-* 'announce, call'); A *mrāc-spāl(yo)*' (with the) head', lit. 'top (and) head', A *mnu-pältsäk*, B *maim-palsko* 'reflection, consideration', lit. 'measuring (and) thinking', A *ak-mal* 'face', lit. 'eye (and) nose'. This latter is extremely interesting in that it is the exact type of Hung. *orcza* (*orca*) 'cheek' from *orr*

'nose' and *szájˇ* (older *szá*) 'mouth', only in reversed order, as W. Schulze had already pointed out.[42] Less significant perhaps are parallels in word formation such as B *werpiśke* 'garden', *werpiśka-tse* 'gardener', *werpiśka-tse-ñňe* 'gardening': Hung. *kert* 'garden', *kert-ész* 'gardener', *kertész-ség* 'gardening'.[43] I shall not repeat all the evidence which Krause presents either on these points or on the question of the parallel declensional forms, which latter is to me less significant, as I have stated above.[44]

We might again look hopefully to the archaeologists for the help which they may offer us in our search for the region of Eurasia where these contacts, which must have endured over a considerable period of time, may have taken place. One is inclined to see some merit in the suggestion by Giacomo Devoto[45] that the Tocharians had a share in the so-called Fatjanovan culture of central Russia at the end of the late Stone Age and the beginning of the Bronze.[46] The geographical location somewhere to the north and west of the confluence of the Oka and the upper Volga is extremely favorable. The Pre-Tocharians could very well have been at least on the southern fringes of this culture, and still in loose contact with the Slavs but in increasingly closer proximity to the Proto-Finno-Ugric community to the east and north. This latter is, I believe, at present located by most scholars to the west of the Urals in the general area of the basin of the Kama and its confluence with the Volga,[47] or even slightly to the south and west of this in the basin of the central Volga itself, including also the confluence of the Oka.[48] Of course, again I must warn that, as far as the people of the Fatjanovan culture is concerned, we have absolutely no evidence as to what language they spoke, nor can we expect to have any. Therefore, let me turn to linguistic evidence again.

If two language groups have been in cultural contact over a long period of time, as the similarities of morphology between Finno-Ugric and Tocharian would appear to indicate, we would expect to find a considerable lexical interchange, and if the former were in greater numbers than the latter, in case their cultural level was more or less on a par, then Finno-Ugric should be the lender and Tocharian the borrower. Unfortunately, for the thesis presented here, evidence of this sort is meager. Possibly the only really significant form to be cited is the Tocharian word for 'gold', A *wäs*, B *yasa*. This form has been generally considered as inherited and is compared directly with Lat. *aurum*, Lith. *áuksas* (old *ausas*), OPruss. *ausis*, and further with Arm. *oski* 'gold' with frequent inconclusive mention of Finn. *vaski* 'copper.'[49] The IE forms could be related by ablaut **aws-* **wes-* from a base **awes-*. The Armenian form must then have *o* from *aw* since *u* would presumably remain,[50] but I am uncertain of the antiquity of the *o* in *oski*. In the case of the Tocharian words, the vocalism is uncertain. From the evidence of the palatalization of *w* to *y* in B the vowel was probably *e*, but our knowledge of such matters in Tocharian leaves much to be desired. However, in spite of my complete incompetence in Finno-Ugric linguistics, I should like to call particular attention to other forms related to Finnish *vaski* cited by Collinder,[51] e.g. Cheremis *waz* 'ore, metal', Votyak *-veś* in

azveś 'silver', *uzveś* 'tin, pewter', Zyryene *-yś* in *ezyś* 'silver', *ozyś* 'tin'. It seems to me that these forms show the probability of an origin common with the Tocharian word, if, indeed, one is not borrowed from the other. I dare go no farther in my speculation.

Krause in the end of the above-mentioned article[52] supplies us with a list of words of the sort which one might expect to be subject to lexical interchange. Among these are especially parts of the body, many of which have no plausible etymology within Indo-European, nor, as far as I can find, do they appear to have plausible connections with anything which one might call Proto-Finno-Ugric. The efforts of A. J. van Windekens[53] to derive these and other words from Uralic or other Asiatic languages, have, in my estimation, met with little success. Of all those suggested, only one would appear tempting, namely, Toch. A *ñom*, B *ñem* 'name', cf. Finn. *nimi*, Votyak, Zyryene *nim*, Hung. *név*, etc.[54] Van Windekens is himself disturbed by the inherited remnants of *n-stem* inflection in Tocharian, e.g. pl. B *ñemna*,[55] which is indeed a serious objection. The vocalism itself points to PIE *o*.[56] The only influence, therefore, attributable to Finno-Ugric influence would be the palatalization of the *n* (cf. also, above p. 16).

Finally, let us consider those words which it is traditional to discuss in studies of this sort, namely the names of flora and fauna. Fortunately, or unfortunately, according to one's predilection for belaboring old cruxes, we do not have the name for the beech to conjure with or to worry about as the case may be. But we do have the word for salmon which, however, means 'fish' in Tocharian! We have skirted the issue already several times. The word is B *laks*, pl. *läkṣi* (masc.). The word has been generally considered as inherited and cognate with the Germanic group represented by OHG *lahs*, Lith. *lāšis*, *lašišà*, Russ. *losos'*, etc. The Finnish *lahi* and Lappish *luossa* are presumed to be Baltic loan words. Since, so the argument goes, the salmon is only found in those rivers tributary to the Baltic, the North Sea and the Arctic Ocean, these words cannot be inherited from Proto-Finno-Ugric, nor can then the Tocharian be borrowed from any Finno-Ugric form.

Paul Thieme has, we know already, devoted much argument to the thesis that these words for the salmon are eventually related to Skt. *lākṣā* 'lac', which in turn is merely the fem. of **lākṣa-* 'red', itself a vriddhi derivation of a presumed **laks(a)-* 'salmon'.[57] That is to say, the word for the red-colored lac is derived from the word for the salmon. It goes without saying that the reverse derivation would be the expected one. The argument is used to support a north European home of the Indo-Europeans. Professor Thieme likewise attempts to find his Pre-Aryan **laks(o)-* 'salmon' in Skt. *lakṣa-* (neut.) '100,000', that is 'multitude, myriad (of salmon)', from the swarming of the salmon in the migration season.[58]

The most important contribution to the salmon controversy in recent years was the brief note by G. Morgenstierne in the *Norsk Tidskrift for Sprogvidenskap* 7 (1934), 120, (bottom): 'Ossetic (Digoric) *läsäg* 'salmon' can scarcely be a loan word from Russ. *losos*! . . . It may be added that species of salmon are found in the Caucasian rivers.' This remark by

Morgenstierne is rejected by Thieme with the words: 'Natürlich kann es sich bei dem in kaukasischen Flüssen vorkommenden "Salmo" nicht um den Salmo Salar, sondern nur um eine Forellenart handeln, . . . ' This statement was further made in spite of his previous knowledge of the entry in W. Miller, *Osset. Wtb.* II. 176: digor. *läsäg*, pl. *läsgutä*, losos', Lachs'.[59]

Most recently, Willy Krogmann in *KZ* 76 (1960) 161–178, 'Das Lachsargument', has contributed a most important and convincing study. For the first time, to my knowledge, the information available about the various species of salmon and their geographical distribution, furnished by competent ichthyologists, is evaluated and applied to the salmon problem. The most important fact emerging at once is that the *salmo salar* is not the only real salmon. There are other species than those which frequent the rivers emptying into the North Sea and the Baltic, and, in particular, there is the *salmo trutta caspius*, the salmon of the Caucasus region, especially of the rivers tributary to the Caspian Sea from the west and northwest.[60] Likewise, a variant of *salmo salar*, the *salmo salar brevipes*, is to be found in the White Sea and its tributaries.[61]

It is abundantly clear then that the Germanic, Baltic, and Slavic names of the salmon, even when coupled with Toch. B *laks* 'fish' or indeed even with a presumed Pre-Aryan **laks(o)* 'salmon', are completely useless as evidence for the north European home of the Indo-Europeans. The eastern border of the salmon territory runs from somewhere around 45 degrees longitude in the north to about 50 degrees in the south, taking in the greater part of southern Russia west of the Urals.[62]

I am not further concerned here with the etymology of the name of the salmon, nor even with Thieme's argument for its existence in Pre-Aryan. As Krogmann has clearly shown, the *salmo salar* is not the only fish in the rivers which may be deservedly called a salmon today and which may have been called a salmon, by whatever name, in Indo-European antiquity. And, in my opinion, the name in question probably did refer originally, not to the *salmo salar* at all, but rather to the *salmo trutta caspius* of the northwest Caucasus region.

There are few other terms of the sort used in studies of Urheimat-problems to be found in Tocharian. We have, of course, words for 'horse', 'cow' and 'dog', namely: A *yuk*, B *yakwe*; A *ko*, B *ke*$_u$; A B *ku*, respectively, and possibly the 'wolf', B *walkwe*, cited in Krause-Thomas (*Toch. Elementarbuch* I, p. 52, 65, 129; II, 239). However, none of these words will help us in our attempt to determine the place of Tocharian among the IE dialects nor tell us anything about the wanderings of the Tocharians through Asia, even if Nehring[63] is right in considering any or all of them eventually of Asiatic origin.

But now let me summarize the conclusions which I think may be drawn from the evidence presented. The earliest affinities of Tocharian remain in general those which Hermann and Pokorny both argued for, and in particular, those which Benveniste later more clearly defined, namely those with Thraco-Phrygian and Armenian. I myself have found little reason to as-

sume close ties with either Hittite or Greek.[64] On the other hand, those more obvious features such as the deponent-passive in *r* and the *ā*-subjunctive, which appear to link closely Tocharian and Italo-Celtic, are late and are due to contacts formed by the regrouping, if we may call it that, of the various languages into later, looser areas of reciprocal influences. The features which it shares with Baltic and Slavic are in part old, due to its midway position between these dialects and Thraco-Phrygian and Armenian to the west and south, but at the earlier period these contacts to the north were far less intimate. On the other hand, at a later period, immediately before the complete separation of Tocharian from its early dialect affinities, they were probably very close and this accounts for the extensive retention of the *ā*-formation in the preterit as well as in the subjunctive. It is, however, difficult to separate the older from the later Tocharian-Slavic isoglosses. One of the former, of course, appears to be the use of the verbal adjective in *-l*. But I shall not here repeat the various data already efficiently assembled by Professor Benveniste in the above-named article.

The last European contact of the Tocharians before the trek across Asia may have been with the Proto-Finno-Ugrians. This might be concluded from the extensive similarities in phonological structure especially, as well as in noun inflection and in word formation as Krause and others have pointed out. The lack of probable lexical interchange between Tocharian and Finno-Ugric remains puzzling, however, and is a serious drawback to this hypothesis. Likewise, as Werner Winter has pointed out in his criticism of my paper, all the apparent structural similarities with Finno-Ugric could well be merely independently developed in Tocharian itself.

Finally, there is no evidence to be deduced from Tocharian which makes a north European home of the Indo-Europeans more probable than it ever was. The 'salmon problem' has been settled once and for all, I hope, by Krogmann. As for the 'beech problem', that is another matter and Tocharian is of no help at all in the controversy.

Addendum

At the Third Indo-European Conference at the University of Pennsylvania, April 21–23, 1966, when this paper was presented orally, several significant remarks on various matters discussed therein were made by other members of the Conference. The original draft has therefore been slightly altered in spots in view of some of these remarks. Due credit, I hope, has been given in case of these alterations. However, there were a few matters which raised a call for more extended discussion.

The most important of these is of course my identification of the *ā*-preterit and *ā*-subjunctive with similar formations in other IE languages. It is both Professor Winter's and Professor Cowgill's contention that this identification is wrong since they hold, I believe, that PIE *ā* becomes Toch. *o* (A *a*, B *o*), according to the evidence of A *pracar*, B *procer* 'brother'. There can of course be no doubt about the derivation here, and other sure instances of

PIE *ā* are rare except for A *mācar*, B *mācer*, but here the influence of A *pācar*, B *pācer* 'father' (with Toch. *ā* regularly representing PIE *ə*) can be invoked. Another certain instance of *ā* would seem to be A *poke*, B (obl.) *pokai* 'arm' (Gk. *pâkhus/pêkhus*) with *o* in both dialects. However, here we find also B (nom.) *pauke* in one instance (74 a 5). In these forms it would appear that we might have to deal with some sort of labial umlaut.

Another example of the equation A *a* = B *o* in a partially similar environment is A *praski*, B *prosko*, *proskiye* 'fright', but here of course the original vowel remains in doubt. Of clear etymology seems, however, A *waṣt*, B *ost* 'house' (Gk. *ástu* 'city', Skt. *vastu* 'place, thing', but Vedic *vāstu* 'dwelling' likewise places the original vowel length in doubt, and again the influence of the following *u* is to be considered). Of even less help is the comparison of A *solu*, B *solme* 'whole' with Gk. *hólos*, Lat *salvus*, Skt. *sarva*.[65]

At least one (to me) certain instance of the development of PIE *ā* (in a monosyllable, to be sure) is found in the nom. sg. fem. of the demonstrative A *sām*, B *sā*$_{u}$ (no matter what the relationship between the added element A *-m*, B *-u* may be).

Another important question involved here is of course the identity of the final vowel of the nom. sg. of adjectives in dialect B like *astarya* 'pure', *rtarya* 'red' (A *āṣtri*, *rätri*), etc., and of the accusative sg. B *astaryai*, A *āṣtäryāṃ*, etc. It is Professor Winter's thesis that consonantal sonant plus PIE *ə* (or rather laryngeal) does not coalesce to form the so-called long syllabic sonants. That is, in the above instances Toch *-yă* reflects PIE *-yə* directly (with final vowel lost in A as is to be expected). It seems risky to me, however, to base such a rule so far as *yə* is concerned on the nom. and acc. sg. of feminine derivative stems in *-yə(ī)/yā*. Rather, I would consider that these have been fused with stems in *-yā* or that, in any case, we are really dealing with a generalized oblique stem in *yā*.

Of significance here too, it seems to me, is the interpretation of the nom. sg. of B *śana*, A *śäṃ* 'wife'. I think it is possible that we have in this highly irregular word the correctly retained nom. sg. PIE **g^{w}enā* (or **g^{w}ṇnā*?). The other ('primary') forms in B are sg. obl. *śano*, gen. *śnoy*, pl. *śnona*. It is clear from the latter that the word is in part fused with *n*-stem inflection (cf. Goth. *qino*, O Icel. *kona*, etc.). Hence, I am unwilling to accept the obl. sg. *śano* as evidence for PIE *ā* > Toch. *o*.

Similarly, I do not accept Professor Winter's derivation of the nom. pl. ending in B *ostwa*, A *waṣtu* or B *ñemna* from *-wə* and *-nə* respectively. Rather, I believe these have P Toch. *-ā* (B *-a*, A-) from plurals of the type of B *-nta*, A *-nt* where the ending was indeed P Toch *-ā* from *-ə*.

I am especially indebted to Professor Cowgill for a most thoughtful letter of May 6, 1966, in which he expounded in some detail his view, expressed in his comments on my paper, that Tocharian shows a particularly close dialectal affinity to Greek. One of his strongest arguments is based on his acceptance of Winter's view regarding the noncoalescence of *yə* to *ī* in both Tocharian and Greek in the above-discussed feminine forms. Another example adduced here is Gk. *zṓō* etc. beside Toch. B pl. 3 *śāweṃ*. His con-

clusion is that both *yA* and *yO* remain uncontracted, but *yE* becomes *ī* as shown by the 'weak' optative sign in both Greek and Tocharian.

In view of the fact that Professor Cowgill's argumentation is not, so to speak, 'in the public domain', but only in a private communication, and not available to other scholars, I hesitate to attempt here to discuss it in detail. In all, Professor Cowgill claims 'a list of six probable or possible Tocharo-Greek agreements. Some of these have been mentioned elsewhere by others and are indubitable I think, as e.g. Toch. *pont-*:Gk. *pant-* 'all', though to be sure the vocalism remains in doubt; another is the 1st sg. mid. ending B pret. *-mai*:Gk. pres. *-mai*, which is to me extremely dubious. It is to be hoped that Professor Cowgill will soon publish the entire list. If then his arguments prove to be acceptable, we will have indeed Tocharian and Greek originally in a much closer dialectal relationship than I have assumed in the above paper.

The value of the Toch. B word for fish, *laks*, in any discussion of the dialectal position of the Tocharians has been brought into question. In the first place Professor Winter has called my attention to an Armenian form *losdi* or *losti* 'salmon' in S. E. Mann, (*Armenian and Indo-European* (*Historical Phonology*) 3). The entry is as follows: '2. IE e followed by an unknown element (cf. 3): Arm. o. *losdi*, *losti* (post-classcal) "salmon" (for *-di*, ef. *di*, *o*, *ov* "body":Oss. (dim. type) *läsäg*:Toch. B *läks* (sic!) ("fish"):OHG *lahs* ("salmon") (etc., the usual cognates) . . . IE *lak̂əs-/lak̂s-*.'

The caption makes no sense to me as it stands. Perhaps one should read 'IE *a*' not 'IE *e*' at the outset. In any case, the word is not otherwise attested in any of the available lexica, and even if genuine, could have little bearing on the probity of the Tocharian form.

In the above-mentioned communication, Professor Cowgill has questioned whether indeed the Tocharian word is actually cognate with the other forms, since he doubts that an IE *a* as generally assumed would give Toch. *a*; rather he would expect *a*, which, of course, would be the 'normal' development of IE *a* in accented syllable. It seems impossible to me, however, to separate the forms, even if we must resort to the suggestion that the Tocharian form is borrowed from another (undetermined) IE source. An Iranian origin, however, appears to me to be out of the question.

Notes

[1] *SSBAW* (1908) 12–13.

[2] Published in *The Johns Hopkins University Circular* 1908–09, No. 9. 106–12.

[3] *Norske Videnskabs-Selskabets Skrifter II. Hist.-Filos. Kl.* 1910, No. 5, Christiania 1911 (Presented at the meeting of the Society on Dec. 2, 1910). 43 pp.

[4] Berlin, 1913.

[5] Strassburg, 1914.

[6] *Skrifter utgifna af kgl. hum. Vetenskaps-Samfundet i Uppsala* 18, No. 4 (1917).

[7] Published as *Die Stellung des Tocharischen im Kreise der indog. Sprachen*, in the *Berichte des Forschungs-Instituts für Osten und Orient* III (Vienna, 1923) 24–53.

[8] *Op. cit.* 53.

[9] E. Sieg und W. Siegling, *Tocharische Sprachreste I. Band. Die Texte: A. Transkription. B. Tafeln* (Berlin und Leipzig 1921).

[10] Halle, 1924, 110–22.

[11] *Ibid.* 116.

[12] *SBBAW* 1924, phil.-hist. Kl. 164 = *Kleine Schriften* 239.

[13] *Det kgl. Danske Vidensk. Selskab. Hist.-fil. Med.* XI. 3 (Copenhagen, 1925).

[14] *Op. cit.* XXVIII. 1 (Copenhagen 1941).

[15] *Germanen und Indogermanen*, 2 vols., ed. by Helmut Arntz, II. 225–40.

[16] *Cf.* infra.

[17] *ZDMG* 71 (1917) 378f.

[18] *Cf.* Meillet, *Esquisse d'une grammaire de l'Arménien classique* 119, 125; Solta, *Die armenische Sprache* in *Hdb. der Orientalistik*, I. Abt, 7. Bd., 21.

[19] *Cf.* Kronasser, *Vgl. Laut- und Formenlehre des Heth.* 201ff.

[20] *Cf.* C. D. Buck, *A Grammar of Oscan and Umbrian.* 179. Examples of the *r*-forms on page 177 and in the verb paradigms pp. 154ff.

[21] Thurneysen, *Handbuch des Altirischen* 309. Conjugations on pp. 347, 359, 367, 405, etc.

[22] Pedersen, *Vgl. kelt. Gr.* 2.403f.; Buck, *Comparative Grammar of Greek and Latin* 251.

[23] Here I am returning essentially to the view of Holger Pedersen in his Schlusswort to his *Tocharisch vom Gesichtspunkt der indoeur. Sprachvergleichung* p. 257, but I have attempted to define more exactly what the wechselwirkung between the Tocharians and the Italiots and Celts was.

[24] On the identity of the *ā* in the *ā*-subjunctive (and preterite), *cf.* further the addendum below, paragraph two.

[25] *Language* 38. 240–53.

[26] *Grundriss* 2, 3, 116ff.

[27] *Vgl. Gramm. d. kelt. Sprachen* 2. 354f.

[28] *Cf.* Maries, *Rev. des ét. arméniennes* 10 (1930) 167ff; Benveniste, *Hirt Festschrift* 2.230.

[29] *Cf.* Stang, *Das slavische und baltische Verbum* 22ff.

[30] *Hirt-Festschrift* 2.225f.

[31] *Indogermanen und Germanen* (1924), 116.

[32] *Cf.* Szinnyei, *Finnisch-ugrische Sprachwissenschaft*, 20f., 22ff.; Collinder, *Comparative Grammar of the Uralic Languages* 45ff., 77ff; Décsy, *Einführung in die finnisch-ugrische Sprachwissenschaft* 156.

[33] It is tempting to try to find some connection between this Finno-Ugric 'Stufenwechsel' and the series of consonants in Tocharian indicated by the so-called 'Fremdzeichen', that is, the consonants before the vowel transcribed *ä*. However, all the consonants, not merely the stops, are found before this vowel, and it is generally assumed that the 'Fremdzeichen' do not indicate a difference in consonant, but rather only in vowel.

[34] Zur Frage nach dem nichtindogermanischen Substrat des Tocharischen, *KZ* (1951) 185–203. Cf. also the brief report of Krause's paper before Der XIII. deutsche Orientalistentag, Hamburg 1951, Sprachliche Beziehungen des Tocharischen zu Nachbarvölkern, *ZDMG* 105 (1955) *68*–*69*.

[35] Cf. especially on possible Turkish calques on Tocharian models as well as Turkish proper names in Tocharian documents, cf. W. Winter, *Ural-Altaic Studies* 23 (1963) 239–51.

[36] Das Tocharerproblem und die pontische Wanderung, *Saeculum* 2 (1951) 225–55.

[37] *Introduction à l'étude de l'Eurasie centrale* (1963) 223.

[38] See footnote 7.

[39] *ZDMG* 71 (1917) 381ff. *Cf.* above.

[40] Professor Gimbutas in the *American Anthropologist* 66 (1964) 893ff. in a Brief Communication has responded to various views of Heine-Geldern's. In her opinion, the 'Tocharian homeland' was central European, that is, to the north and west of the area I have indicated in my paper. She is also of the opinion that Heine-Geldern is in error in finding any connection between the Tocharians and the Cimmerians. Mrs.

Gimbutas, in her oral comments on my paper and later in a personal letter, rejects also Devoto's suggestion of contacts between Tocharians and the Fatjanovan culture. It is her view that the Tocharians migrated, 'together with the Phrygians and Armenians, across the Balkans and Anatolia around 1200 BC.' If so, then they must have followed a more southern route to their eventual eastern homeland. I feel I must state, however, that as long as a people leaves no written records in any area through which it passes, it is extremely difficult to produce evidence that they were ever there at all.

[41] *Op. cit.* 198f.

[42] *Kleine Schriften* 248ff. (esp. p. 256) = *Ung. Jb.* 7 (1927) 168–97.

[43] Krause, *op. cit.* 196f.

[44] Professor Winter calls attention to the fact that, after all, the Tocharian (and the Hungarian) series do not differ greatly from the German Garten, Gärtner, Gärtnerei!

[45] *Origini indoeuropee* (1962) 359.

[46] *Cf.* footnote 40.

[47] *Cf.* Décsy, *op. cit.* 215ff.

[48] *Cf.* Y. H. Toivonen, *Suomalais-Ugrilainen Seura* 56 (1952) 1–41.

[49] *Cf.* Benveniste, Hirt Festschrift 238f.; Pokorny, *Indog. etym. Wtb.* 86f., etc.

[50] Meillet, *Esquisse* 42, 44.

[51] *Finno-Ugric Vocabulary, An Etymological Dictionary of the Uralic Languages* 66.

[52] See footnote 34.

[53] For example in *Orbis* 11 (1962) 600ff.; 13 (1964), 589ff. Van Windekens' methodology leaves something to be desired since he appears to pick up forms of any age from available lexical compilations without attempting to reconstruct the form of the word at the time when the Tocharians may have borrowed it. Given the present state of our knowledge of the history of the Altaic and Hyperborean languages and of Ainu, this is of course very dangerous.

[54] For the other forms cf. Collinder, *op. cit.* 39.

[55] *Op. cit.* 607. Read *ñemna* for *ñemma*.

[56] *Cf.* also Krause-Thomas, *Tocharisches Elementarbuch* I.54.

[57] Der Lachs in Indian, *KZ* 69 (1951) 209–216, esp. p. 210; *Die Heimat der indogermanischen Gemeinsprache, Akademie der Wissenschaften und Literatur in Mainz, Abh. der geistes- und socialwissenschaftlichen Kl:* Jg. 1953, Nr. 11. *Cf.* especially pp. 253ff.

[58] *KZ* 1. c.

[59] *Abh. Ak. Wissensch. u. Lit. Mainz* 1. c. p. 557.

[60] Krogmann, *op. cit.* 166f.

[61] *Ibid.* 164.

[62] *Ibid.* 178.

[63] Studien zur indog. Kultur und Urheimat, in Die Indogermanen und Germanenfrage, neue Wege zu ihrer Lösung. *Wiener Beiträge zur Kulturgeschichte und Linguistik*, Jg. IV (1936) 106ff.

[64] *Cf.* however Professor Cowgill's arguments reported in the addendum

[65] *Cf.* Krause-Thomas, *TEb* I: 54, 55; Pedersen, *Tocharisch*; Frisk, *Gr. etym. Wtb.* II, 1831.

The Indo-Europeanization of Greece

William F. Wyatt, Jr.

The difficulty we face in our attempts to reconstruct the early history of a people is twofold. On the one hand the archaeological evidence is silent—the mute stones do not in fact speak—while on the other, linguistic evidence, though not silent, is by its nature timeless and unlocalizable. Hence it may seem at first blush that the two facets of our knowledge must ever, like Janus, face in opposite directions. And so it has frequently seemed in the past. But repeated attempts at a reconciliation and synthesis have been made, particularly with a view to establishing the place and period of the Indo-European linguistic unity. Rather than approach this important and always interesting problem, I shall in this paper try to utilize both linguistic and archaeological evidence in a more limited area, the peninsula of Greece. In so doing I shall discuss the question of the date of the arrival of the Greeks in Greece, a question which has exercised scholars for quite some time, and has not yet found an answer that has proved satisfactory to all.

When did the Greeks arrive in Greece? Estimates vary over a millennium from the Early Helladic II period ca. 2500 B.C. down to the Late Helladic period ca. 1500 B.C. Many scholars agree on 1900 (the date of the end of Early Helladic III [=EH III] and the beginning of Middle Helladic I [=MH I]), but there is enough dissent from this view to warrant a new investigation of the problem. There are in principle three ways of approaching the question: the historical, the archaeological, and the linguistic. If one were to apply the historical method pure and simple, he would take all Greek legends purporting to deal with Greek prehistory, sift them, and try to determine what, if any, truth is revealed or concealed by them. In fact this is impossible, for myths are unreliable and mutually contradictory, and all historians consider the archaeological evidence primary, whether or not they also consider legendary evidence. Also relevant to the historian are the Hittite and Egyptian records of the fourteenth and thirteenth centuries B.C. which very likely mention the Greeks (Page 1959:1ff.). But these records are too late to affect our problem. The archaeological evidence is of course that evidence which emerges from the ground as the result of excavation, and is a necessary basis for any other approach. It tells us the succession of building periods, destructions, types of artifacts, pottery sequences and the like, and thus indicates when changes in culture may have

taken place. But of and by itself the archaeological evidence tells us nothing about the movements of people, save by inference, and is therefore incapable of providing an answer to the question set at the beginning of this paragraph. One might then hope that the linguistic evidence would provide us with a sure means of determining the date of the arrival of the Greeks. And doubtless it would, if we had written records of sufficiently early date. But then of course it would be historical evidence, and in any event we do not have such written records, and must therefore put up with something less than ideal certainty. Indeed we shall not hope for a conclusive answer to the question, but shall hope to circumscribe it somewhat, and try to establish those limits, upper and lower, within which it is likely on linguistic grounds that the Greeks arrived in what was to be their later homeland. After having completed our discussion on these grounds we can then confront or compare our results with those of the prehistorians who bring important results to bear from another angle.

A question of this sort demands definitions, for without them numerous answers can be given without providing any real information (Chadwick 1963:14–15, Pulgram 1960). First we must establish the people of whom we are speaking. We do not care what their blood type was, nor whether they were dolichocephalic or otherwise, and in fact do not care about them as people at all. We as linguists are interested in them only insofar as they are conveyors of Indo-European speech into Greece, an Indo-European speech which thereupon merges with one or more non-Indo-European languages such that a language emerges which has a phonological system including voiceless aspirates [p^h t^h k^h], an opposition between long and short vowels, etc.; morphophonemic rules whereby //t^h . . t^h// appears phonemically as /t . . t^h/, and preconsonantal /s/ alternates with /h/ save in the environment //h . . t^h// where //h// disappears; a morphological system containing a noun suffix *-eús* used to form both agent nouns and personal names, an optional augment of past indicative tenses of the verb, a perfective (stative): imperfective opposition in the verbal system, etc.; a lexicon which contains such items as *thálassa* 'sea' and *asáminthos* 'bath-tub' as well as *híppos* 'horse' and *anḗr* 'man' (cf. Chadwick 1963: 5ff.). In other words, we are concerned with that language which is later called Greek, and explicitly exclude from consideration any earlier Indo-European languages which may have been present in Greece, such as Pelasgian, without taking a stand on the likelihood of hypotheses involving them.[1]

By Greece we shall mean mainland Greece, that is, the Peloponnesus, Attica, and that portion of Greece north of the Gulf of Corinth which later was included in Boeotia, Thessaly, Locris, Phokis, and Aetolia. By so doing we exclude not only Crete and Cyprus (and a fortiori Magna Graecia), but more importantly for our purposes, Asia Minor, and with it the question of the linguistic affinities of the inhabitants of Troy VI (Blegen 1961:10, Page 1959:56). The when is of course the thing to be decided, but certain limitations, concerned rather with the how, can be placed on it now. It no longer seems necessary to assume, as Kretschmer (1909:9ff.) and Nilsson (1933: 72ff.) did, three waves of Greek invaders. In fact it seems more economical

and more likely that there was but one invasion, and that we should think of the arrival of the Greeks as a sudden incursion, or if not that, at least as a gradual trickling in of settlers following a rather larger push. In other words, I accept Chadwick's arguments (Chadwick 1963:14–15) for a single invasion of Greece, and a subsequent split into dialects on Greek soil. The Dorian invasion, though doubtless a disruption of established life at the time, need not be imagined as an invasion from beyond the Balkans of a separate Indo-European-speaking tribe. There is much against such an assumption, and much for assuming the opposite. After all, Doric is sort of a conservative country Greek, intelligible, though scarcely polished and dignified, to all other Greeks.

Two points, one fixed, the other movable, can be established at the outset. Greek is an Indo-European language, and clearly cannot have come into being as a separate language until after the dissolution of the earlier speech community of which it formed a part. This is our upper limit, but unfortunately it is not a fixed point, for we do not know exactly when the Indo-European speech community existed.[2] The other point, this one fixed, is provided by the Linear B inscriptions from Cnossos on Crete and from Pylos and Mycenae on the mainland. These inscriptions, written in Greek, date at Pylos from about 1200 B.C., and at Cnossos from about 1400 B.C. Hence Greek speech as defined above must have come into existence at the latest by 1400 B.C., and doubtless is in fact somewhat older. 1500 would seem to be the absolutely lowest date possible.

But what is the upper limit? Here opinions of course differ, but there are only a few possibilities that can even be mentioned for the date of arrival of Indo-European speaking peoples into Greece, and of those only those involving dates of 2200 and later are really strong. No one, so far as I know, has felt that the inhabitants of Greece during the Neolithic period (?–2900) were speakers of an Indo-European dialect. What they did speak is of course unknown and unknowable, unless one wishes to assign to them various non-Greek names of geographical features such as mountains and rivers (Vermeule 1964:18–19). Hence we do not know what they called the plants, animals, and artifacts which they knew and used. The other possibilities cannot be excluded a priori.

The year 2900 marks the beginning of the first and earliest subdivision of the Greek bronze age (Early Helladic I [=EH I]), a period in which 'metal was passing out of the rare-ornament category of discoveries into the category of economic necessity' (Vermeule 1964:25). The Greek Bronze Age was a result of the technological advances in the Near East which brought about ca. 3000 an increase in wealth and population. Whether we owe the beginning of the bronze age in Greece to colonization from Anatolia is uncertain (Vermeule 1964:26), but it was the impetus from the Near East which caused the inhabitants of Greece to give up their old ways and adopt a newer technology. Of linguistic evidence there is none, and no one has suggested that the Greeks were present this early. But since it is at least theoretically possible, I mention it for completeness' sake.

The beginning of Early Helladic II dates about 2500 B.C. It was during

EH II that the Early Helladic culture reached its height, particularly at Lerna in the Argolid. Architecture of a monumental sort is there represented by the 'House of the Tiles', a building begun at a time when stamped seals had already been introduced and when metal tools were well developed. There was definitely a cultural flowering at this time, and it is customary to attribute such flowerings to a new people. Schachermeyr (1954:1443) thinks of an Anatolian invasion; Weinberg (1965:304) of an influx of peoples from the Cyclades; while Vermeule (1964:63) thinks of an Indo-European tribe, though not of Greeks.

The year 2200 is the beginning of the latest phase of the Early Helladic culture, EH III. Everyone is agreed that there was at this time a new and destructive invasion of Greece, or at least of the Argolid, though its extent was not known until 1960 (Caskey 1960). Many sites were destroyed at this time, but they were rebuilt by the conquerors, and then survived without interruption down into the Mycenaean period. They escaped the destruction associated with the beginning of the Middle Helladic I period. These peoples are the first to have a serious claim to being considered Indo-European, though so far as I know only Chadwick (1963:15) and Samuel (1966:37–9) among classicists have mooted the possibility that they were Greek speakers. Vermeule (1964:31), however, alludes to current theories which 'explore the possibility that all three groups (sc. EH I–II, EH III, MH) were Indo-European and that the Greeks of the late Bronze Age had a complex variety of relatives and forerunners in the land before them.' Likewise Caskey (1960:302) wonders whether 'the people of Early Helladic III may not have been closely akin to the Middle Helladics and thus also of direct or indirect parentage to the Mycenaean Greeks.' Weinberg (1965: 305), while avoiding the title 'Greeks', is more definite: 'I believe there can now be little doubt but that the first wave of Indo-Europeans reached the Peloponnesus between 2300 and 2200 B.C.'

The year 1900 marks the beginning of the Middle Bronze Age, an age ushered in by burnings at a number of the sites that were not burned at the beginning of EH III, and also by the introduction of a new pottery style known as Minyan Ware. Whether the burnings and the pottery are contemporary or not is still a matter of dispute, but they are at least close in time, and seemingly due to the same people. Little else is due to these people, however. The early stages of the MH period are poor, days of poverty with meager and unimpressive architectural remains. The sites are usually the same as those of the EH period, and are unwalled, small, and crowded with buildings set without an overall plan wherever the owner wished to place them. If it was an invasion of new people to which we owe this destruction of the EH sites, it was not a glorious invasion of superior people, but perhaps rather the overwhelming of an earlier civilization by people superior only in numbers.

If there is a canonical, generally accepted date for the arrival of Indo-European speech in Greece, this is it, and has been since the important

article of Haley and Blegen (1928:141–154). It has been accepted by Schachermeyr (1954:1489–94), Wace (1956:xx & xxvii), and Dow (1960:3) to name but a few influential names. The arguments presented by Haley and Blegen in support of this date are impressive, and still convince most archaeologists (Vermeule 1964:61). It is therefore important to understand what these arguments are. They have to do essentially with two factors: the distribution of pre-Greek place names, and the occurrence of destruction levels at prehistoric sites in Greece.

There are in Greece a number of place names such as Corinthos, Tiryns, Cnossos, and mountain and river names like Erymanthos, Lycabettos, Ilissos, all of which have in common non-Greek bases and non-Greek suffixes, generally, or at least frequently, containing *-ssos/-ttos* or *-nthos*. These names are spread all over mainland Greece, the islands, and Crete. The assumption is that, since they are non-Greek names, they must have been given the localities and features in question at a time prior to the arrival of the Greeks. If these place names can be correlated with material remains, we should be able to form an approximate idea of when the names were given. That culture which correlates with the place names will be the one which gave them, and the one in whose language the names will be meaningful. This evidence will in turn provide a terminus post quem for the arrival of the Greeks.

Only two periods in Greek history would allow for the actual distribution of these names: the EH period, and the period LH III = LM III after the fall of Cnossos (ca. 1400 B.C.). During the Neolithic period the islands were not inhabited, and hence the names cannot have spread to them then; and during the MH period the cultures of Greece and Crete diverged. There was no common culture, hence no common language, and therefore the names cannot have been given then. LH III = LM III is much too late, for we know that there were Greeks in Crete at that time, or had been just previously. Hence the names can have been given no later than EH III, and the Greeks cannot have arrived before MH I.

There are numerous destructions at the time of the transition from EH III to MH I in much of Greece, and since there are no destructions of any importance after that until the end of the Mycenaean period, LH III, there is evidence for the arrival of a new people only at those two historical points. Again the date LH III is far too late, and by elimination, the Greek language and the Greeks can have entered Greece no later than the beginning of MH I, a period of widespread destruction on the mainland, but no destruction on Crete or the Islands. Hence they arrived at the beginning of MH I. Connections with Troy serve to buttress the argument, an argument based on the assumption that people enter a new area only by destroying the previous civilization.

Though these two arguments are plausible, they are not conclusive, as I think even their authors would grant, and their hypothesis must remain provisional only. Objections do occur. (1) First, the place name argument

rests on a number of assumptions that do not necessarily hold. One of these is that people do not carry names from one spot to another when they move. A glance at any map of any state in the country will serve to dispel this illusion. In Eastern Massachusetts many of the names, such indeed as Medford and Boston, were imported from England. Natick and Winthrop were not, but are not for that of the same language family. (2) Secondly, and more important, it is assumed that the *-ssos/-ttos* names and the *-nthos* names belong to the same linguistic group, an assumption equivalent to assuming that Milwaukee and Racine were of the same language family because not of the family which includes Madison and Appleton. Schachermeyr (1954:1512) endeavored to provide the necessary proof of this, but his arguments are insufficient. In fact if one looks at the maps in his article (1954:1501–2, 1503–4), one will find that the distribution of the *-ssos/-ttos* and *-nthos* names differs somewhat: *-ssos/-ttos* names tend to appear in Attica, northern Greece, the coastal Peloponnesus, and near Cnossos; while *-nthos* names are more common in the inland Peloponnesus. If this impression, for it is only an impression, is borne out by a more thorough investigation of the evidence, then the unity of culture argument falls down. It is, for instance, eminently possible that peoples from mainland Greece settled the Cnossos area (and here and there around the coast) after the period when *-ssos/-ttos* names were first assigned to localities in Greece. In this event the area of cultural unity reflected by the names will not include Crete, and the MH period opens up as a possibility, while the EH period is excluded. It takes as little as this to weaken the argument based on the unity of culture. Too, these are not the only types of names included. Almost all other clearly non-Greek names are included in lists of pre-Greek place names and assumed to stem from one single source. But even if we were to grant unconditionally the validity of the arguments presented, the conclusions are still essentially negative, and hence are incomplete. (3) In order to complete them, we should need also to investigate the names of all subsequent settlements in Greece to determine what names were given *new* sites established in MH and LH times, particularly the place names revealed by the decipherment of Linear B. The first new site to bear a Greek name will provide evidence for Greek habitation. And conversely, if we find MH sites with non-Greek names, we may assume, hesitantly and with all due caution, that it was other than Greeks who established the place. One thinks immediately of Crisa in Phocis and Eleusis in Attica, both MH sites, and both bearing non-Greek names (Hampl 1960:69). (4) Furthermore, most of the EH sites with *-ssos/-ttos* and *-nthos* names continued to be occupied down into the classical period: otherwise we should not know their name. In order to accept as binding the Haley-Blegen argument, we should have to assume also that towns never change their name, an assumption of no validity whatsoever. Tiryns, impressive in EH II, was unimportant until the MH period; is it not possible that it received a new name to go with its newfound power? Boston once was called Shawmut. (5) A further difficulty,

the import of which I am unable to assess, is the fact that names in *-nthos* and *-ssos* occur not only in Greece, but in Asia Minor as well, and to a lesser extent in the Balkans and Italy (maps: Schachermeyr 1954:1501–12). If we take the unity of culture argument at its face value, we should have to seek a period in which the entire northeast Mediterranean shared the same or a similar culture. I do not know when this could have been, though the MH period and EH III seem most likely. But if we opt for MH, then the Greeks cannot have come in at that time, for the names are clearly non-Greek and non-Indo-European. EH III would suit the Haley-Blegen argument and date, but not an EH III arrival date for the Greeks, again because the names are not interpretable as Indo-European. And yet the EH III period would seem the most reasonable from the archaeological point of view, for these names occur all over Asia Minor and Greece, and the early destructions in that area took place at that time (Weinberg: 1965:305). The unity of culture argument thus leads to difficulties, but if we give it up, we shall have to abandon also the conclusions drawn from it.

The place name argument is not airtight, and the best that can be claimed for it, as Cowgill points out to me, is that it fails to contradict the archaelogical evidence for the arrival of the Greeks toward the end of EH III. In any event the chronological conclusions drawn from it should probably be modified in the light of Caskey's arguments concerning the EH III destructions in the Argolid, though neither Blegen (1962:11,30) nor Mylonas (1964:7) have felt it necessary to change their views. But the logic of their argument would seem to demand that they change them, for the unity of culture can have existed only in EH II or earlier (Caskey 1960:302).

The second prong of the Haley-Blegen argument, namely the assumption that destruction of sites necessarily involves a new people, and conversely that no destruction means no new people is of course a priori and can be accepted or not. Archaeologists commonly accept it, though Wace (1958:33) seems an exception, but linguists such as Palmer (1965:349) and Buck (1926:2) are skeptical and prefer to allow for the possibility that a new people, particularly one at a lower cultural level, may appear on the scene without destruction of sites. I shall, perhaps hubristically, join Palmer and Buck, and assume that a new people may enter either without or with the destruction of earlier settlements.

The dates 1600–1500, chosen as likely here, vary over 100 years, and correspond roughly to the beginning and end of the shaft-grave period at Mycenae, a period which, beginning in MH III (late Middle Helladic), includes the entire Late Helladic I period. Nothing in preceding centuries has prepared us for the glory and splendor of the civilization revealed by the shaft-graves. We have to imagine either that a sudden impetus to create marvelous and rich works of art suddenly caused the inhabitants to produce the daggers, death-masks, and the like of the shaft-graves; or that a new dynasty at least, if not a new people, has come upon the scene. Mrs. Vermeule (1964:110) is uncertain whether the princes of Mycenae

were 'an elite which rose from the old Middle Helladic Indo-European stock in response to new opportunities, or a group of related aliens who made a peaceful entry to the Argolid.' Stubbings (1963:9ff.), stressing the undoubted Egyptian connections of many of the artifacts, feels that an Egyptian dynasty has come upon the scene. Palmer (1965:348–9) would apparently leave the shaft-graves to his Luvians.

Linguists seem to favor this period for the arrival of the Greeks, generally for the reasons advanced by Pulgram (1960:171), namely that this is the earliest date for which we have any reliable evidence of Greek speech. Chadwick, though he later admits as possible a date as early as 2100 B.C. (1963:14–15), states that the lowest possible estimate of the origin of Greek must be put well before 1500 B.C. And Buck (1926:24) arrived at 1600 B.C. as a probable date even before the decipherment of Linear B. L. R. Palmer (1965:348–9), who feels that Greece during the MH period was inhabited by Luvian-speaking Anatolians, places the arrival of the Greeks at about 1500, though at other times he seems willing to admit a date of 1570, contemporary with the beginning of LH I.

For Palmer and Pulgram, at least, two suppositions are basic to the dating. Linear B is Greek, and hence Greek speech can have entered no later than 1500 B.C., and furthermore, it need not, on linguistic grounds, have entered any earlier, if we are willing to grant that Linear B can have developed from proto-Greek in so short a time (100 or 300 years).[3] Buck's argument is essentially similar, though he introduces the distribution of Minyan pottery as an objection against a northern invasion, the presumed direction from which the intruding Indo-European speakers arrived. Palmer (1965:321ff.) adopts the Haley-Blegen argument, namely, the distribution of place names, and accepts the identification of the inhabitants of Troy VI, founded ca. 1900 B.C. with the MH invaders. But he concludes that these people were Luvians, not Greeks. And since Luvians were in control of Greece during the MH period, the Greeks cannot have entered until the next break, which he considers to be the beginning of the LH period. His arguments have failed to convince most scholars (Mylonas 1962:284–309, Schachermeyr 1962:27–39), who object to Luvians in Greece. But objecting to Luvians does not of course necessarily involve rejecting Greek invaders at the beginning of LH I. Palmer provides no positive evidence for his date of 1500.

Nilsson (1914:522ff., 1933:72ff.) was concerned to show that LH I culture and in general all Mycenaean culture was different from the Minoan, though very strongly influenced by it. He was, therefore, not primarily interested in the date of the arrival of the Greeks. It was his belief that the Greeks arrived in three waves, the first of which was the Ionian at the beginning of LH I, the second the Achaean toward the end of LH II, and the third of course the Dorian which came in only to destroy at the end of LH III. He was able, he thought, to show that there were characteristic differences between the cultures of Late Helladic I and Late Minoan I, which showed that Crete did not control the mainland during that period. Most of his arguments—having to do with megaron, amber, the chiton, as well as a warlike

spirit, and a passion for the monumental—which served to reveal the differences between the Mycenaeans and the Minoans, are valid enough by themselves, but do not necessarily indicate a new people in Greece. The megaron, for instance, is simply one way of constructing a room, a simple one, the antecedents of which reach back as far as Neolithic B in Dimeni in Thessaly. But he did mention one matter which merits attention. He pointed out that the horse was introduced into the Greek world first in Mycenae, and that it appeared in Crete only later. Here is a real cultural difference which might tend to show the arrival of a new people, not perhaps by itself, but in company with the war chariot which first makes its appearance on a stele from grave circle A of Mycenae, and on a gem found in grave 4 of the same circle. Apparently horse and chariot appeared at the same time on the mainland, but only later in Crete. This does not of and by itself prove the arrival of a new people, but strongly indicates it.[4]

But for the horse (and chariot, which he does not mention) Nilsson's arguments are unconvincing, for there is nothing that cannot have been brought in by trade, and nothing that must have been brought in by a new people. And Schachermeyr (1951:705–51) has endeavored to take that one item away by arguing that the battle-chariot was introduced into Greece from Egypt after 1570 B.C. when the Greeks came in contact with the Hyksos. He states (1954:1491): 'Er kann also mit der Einwanderung der Griechen ebensowenig zu tun gehabt haben wie die gleichzeitig von Kreta her einwirkende minoische Kultur.' Hence, though a cultural innovation, the chariot, too, entered by cultural assimilation and not with marauding bands of hostile invaders. But as for the chariots, if they were introduced from Egypt, they cannot have been introduced by Greeks who previously did not know the chariot, for many words connected with the chariot are of Greek formation and Indo-European origin. Had the Greeks first encountered the chariot in Egypt, they would almost certainly have done one of three things: they would either have used borrowed terminology, the most likely option in my opinion; or they would have used Greek words calqued on the native Egyptian terms; or they would have used Greek terms normally used for objects which could be felt to bear a relation to the chariot: say, words for various conveyances already known to the Greeks, or words drawn from some other sphere of transportation, perhaps shipping.[5] We can either agree with Schachermeyr that the chariot came from Egypt to Greece, but was brought by other than Greeks; or that the chariot, together with its terminology was introduced by Greeks at 1570; or that the chariot was introduced then, but the Greeks came in later in their own chariots with their own terminology. In the first and third cases the Greeks cannot have arrived as early as 1570. In the second case they will have arrived at 1570, or at least at the same time as the chariot. In any event, not so early as 1900. We shall return to the chariot below.

The year 1400 corresponds to the date of the destruction of Cnossos, and marks the transition from LH II to LH III. We have seen above that Nilsson would bring in his Achaeans at this time, but no one to my knowl-

edge has seriously proposed this as a date for the arrival of the Greeks as a whole. And if the usual dating of the Linear B tablets from Cnossos is substantiated, clearly this date is out, for Greek, according to this view, was spoken at Cnossos prior to 1400. But if Palmer (1965:171ff.) should be right in down-dating the Cnossos tablets to ca. 1200–1150, then the LH III period will open up as a possibility for the introduction of Greek. Pending a final decision from the archaeologists on the Cnossos question, I will accept the standard view, and not here explore the possibility of a LH III date.

We have now seen the types of arguments that have been adduced in the past, and the conclusions which they have been called upon to serve. It is now time to turn to the linguistic evidence. It may well be wondered, in the absence of written records, what answer the linguistic evidence can give to our question. Indeed, what linguistic evidence is there? It must be admitted that there is not much, but there is some, and that evidence is precisely the Greek language, particularly that language in its earliest phases, that is, Mycenaean and Homeric Greek. We shall assume that the following postulates have some validity, though they are far from having the status of laws. The first two in fact serve merely to limit the applicability of the third.

1. A borrowed word, unless it is a technical term or part of a technical jargon, may denote an object not previously known to or noticed by the borrower. Examples of this are E. *street*, G. *Strasse* both from L. *strāta*; E. *tile*, G. *Ziegel* both from L. *tēgula*. But this is not at all certain, and to ensure certainty, perhaps should be phrased negatively: a word is not borrowed unless the thing designated by that word is at the time of the borrowing known to speakers both of the source language and of the borrowing language. It may or may not have been known previously to the speakers of the borrowing language.[6] The fact that the Greeks called 'ass' *ónos* and 'gold' *khrusós* indicates that they knew neither of these things when they came in contact with speakers of another language who did. The mere fact of borrowing is not criterial here, however, for the Greeks innovated also in using *khalkós* for inherited **ayos*, but rather the fact that there are no Indo-European words for these things. The evidence of two borrowings, *peírins* and *lépadna*, to which we shall return below, is not so certain, for the Greeks may have already known these objects, or a variety of them, but adopted the words as technical terms.

2. A new formation created from native linguistic resources (or from native plus foreign resources) will probably denote an object not previously known, and may in fact represent a calque. One thinks here of such E. words as *gooseberry* < **grozeberry*; *sparrow-grass* besides *asparagus*; *crawfish* < *crayfish* < *crevise*. The same can be said of metaphorical uses of native words. Examples of this sort of thing are of course frequent and well-known: cf. G. *Fernsprecher* 'telephone' and Greek /tileórasi/ 'television'. But this criterion is of dubious value, and perhaps can serve best to provide a limit to the next criterion, that dealing with inherited words. Not all inherited words will be of equal weight, and compounds and metaphorical uses will be rated low as evidence.

3. This is the most important, for it provides a terminus post quem. An inherited word with its original meaning denotes an object or concept constantly and continuously known to the intruder which may or may not have been present to the experience of the earlier inhabitants. Examples and applications:

a. The Greeks called 'sheep' /óis/, a word derived from an earlier */ówis/, and because of Latin /owis/, Sanskrit /ávi-/, Luvian /hawi-/ clearly of Indo-European provenience. Sheep were known in Greece already in the Neolithic period, so there was a non-Indo-European word available for the Greeks to borrow had they wanted to. But they did not, thus proving that they had known sheep already in the place whence they came. But even had they given up the word */ówis/, we would still have secure evidence that the Indo-European intruders had sheep because of *arḗn* < **warēn* 'lamb' and various words having to do with wool, all words intimately concerned with sheep, and all inherited terms.

b. But even if there had been no sheep in Greece, and the conveyors of Indo-European speech also brought the sheep, we would still expect the later Greek word to be */ówis/. It would have been the only term available.

c. The third case requires oversimplification and is at least in this instance based on a hypothetical example. If we were to imagine that some Indo-Europeans possessing the word /ówis/ 'sheep' migrated without sheep to an area in which there were no sheep, they would either soon forget the word /ówis/, or would apply the word to a previously unknown, though possibly sheeplike, animal.[7] Then clearly, if they came upon the sheep later, they would either borrow a word if one was available, or make up a new one from their native linguistic resources. They would in any event not call the sheep /ówis/, unless they happened to borrow the word from a linguistic group which also called 'sheep' /ówis/. This case, then, is simply a different instance of categories (1) or (2).

The point of the foregoing is: if we can attach a Greek (IE) name to a cultural innovation on Greek soil, either the IE invaders came at the same time as that innovation, or they found it already there when they arrived, and themselves possessed the object before they arrived. They cannot have preceded it.[8] Thus, if the sheep had been introduced into Greece ca. 2000, and if the Greeks called it /ówis/, then IE speech cannot have entered Greece before 2000. If it had, then those people would have known the sheep by some borrowed name, a word learned at the same time as they became acquainted with the animal.[9] But we must be as certain as we can about the soundness of the evidence on which we base our conclusions. Words are lost for subtle reasons, and of course they change their meaning sometimes quite dramatically. These are matters over which we have no control, and in order to minimize their effect, we must treat not of individual words only, but where possible of words which, though they refer to discrete objects, yet have in common the fact that those objects are customarily associated. An example might be, for instance, the plow and its parts, all of which have a name. And though it is likely that one or two

words connected with the plow will be replaced or change their meaning, it is not likely that all will. Hence any IE word in Greek connected with plowing will indicate, though not prove, that Indo-Europeans had the plow, and that they brought knowledge of it with them to Greece.

The best place to begin our search for areas of meaning containing Indo-European elements is clearly the earliest source of information concerning Greek, namely the Linear B tablets of Cnossos, Pylos, and Mycenae. Though these tablets are in many respects disappointing, in that they provide little or no information about many of the topics in which scholars are most interested, they do at least have the distinct advantage of treating in some detail those matters in which the scribes were interested. Many times this means simply that we are presented with lists of men's names, but other times it means that we have a fair amount of information about the Mycenaean vocabulary in a certain area.

One such area is that of warfare and military equipment. There are a number of tablets found at Cnossos concerned with chariots and their parts. That these tablets deal with chariots is quite clear, for there is always included at the right edge a drawing of a (generally wheelless) chariot. We thus have a reasonable check on the vocabulary items, and can be fairly certain of our identifications with Greek terms. KN Sd0403 reads (*Docs*: 365–66):

> [i]-qi-ja/a-ja-me-na e-re-pa-te a-ra-ro-mo-te-me-ṇạ a-ra-ru-ja [a-ni-ja-pi] e-re-pa-te-jo o-po-qo ke-ra-ja-pi o-pi-i-ja-pi 'ko-ki-da' ọ-pạ'
>
> WHEEL-LESS CHARIOTS 3

Ventris and Chadwick translate: 'Three horse-(chariots without wheels) inlaid with ivory, (fully) assembled, equipped with bridles with *cheek-straps* (decorated with) ivory (and) horn *bits*. The *feudal contribution* (of) Kolkhidas.' (Words in italics indicate uncertain interpretations.) Now it is clear that not all the words in this text are securely interpreted, let alone interpreted as Greek, but it is also clear that some are transparently Greek, *i-qi-ja*, for instance, clearly is a derivative of *i-qo* (= *híppos* 'horse') and doubtless here means 'horse-chariot', a form that may lie behind the strange Homeric compounds *hippiokhármēs* and *hippiokhaĩta* (Morpurgo 1963: 115 with references). It may modify an (unexampled in Mycenaean) *amaksa*, as the authors of *Docs.* suppose (*Docs*:365), or more likely *wo-ka*, a word that appears on the wheel tablets, to which we shall return below. The fact that *i-qi-ja*, an adjective, is used indicates that there was some other type of *wo-ka* available in Mycenaean times, and that *wo-ka* would almost certainly have been one drawn by mules. (Cf. Homer's insistence in *Iliad* 24.266ff. that both the cart and the yoke were those appropriate for mules.)

a-ni-ja-pi appears elsewhere, e.g. KN Sd0401, and is an instrumental of the *a-ni-ja* that occurs on Sd0402: *o-u-qe a-ni-ja po-si* 'and there are no bridles attached' (*Docs*: 367). It obviously is the same word as the Homeric *hēnìa* and the classical *hḗníai*. Indo-European connections for this word are

not altogether certain, but are probable. Ventris and Chadwick (*Docs*:265) following Boisacq (1923:325), connect it with Sanskrit *nāsyam* 'bridle passed through the nose'. Frisk, however (*GEW* 1.637) identifies it with the Irish plural *ē(i)si* 'reins, bridle' (<**ansio-*), and connects it further with other words meaning 'handle' and the like, such as Lat. *ānsa*, ON *æs* f. (<**ansiā*). Whatever the case, Indo-European connections seem probable.[19]

The other nouns are less certain, for they do not correspond to classical terminology. It is therefore possible either that they are not Greek words at all, or that they are calques on non-Greek words formed by means of Greek lexical resources, in any event innovating terminology. This could mean either that the Greeks did not originally know these objects, or that as they adopted a new and better version of their old equipment, they picked up a new name for it as well, whether borrowed or made of native linguistic elements. Plausible Greek connections can be made for at least some of them.

o-po-qo is not certainly interpreted, and the only reasonable suggestions thus far made are Palmer's, recorded in *Docs*:364–5, and Chadwick's (1966: 429). Palmer feels that *o-po-qo* is a 'cheek-piece', a necessary part of any harness arrangement. If his identification is correct, then we may approve his analysis of the word as *op-ōkʷon*, a compound of *opi* and a thematic derivative of **ōkʷs* 'face'. He compares *parṓpia* 'blinkers' found in Pollux (2.53, 10.54) and the Suda. But Palmer's interpretation seems unlikely (compare the critique in *Docs*:365), the more so since Homer has *parḗion* (*Iliad* 4.142) for what must have been essentially the same object: it seems unlikely that two words would have been coined for this one object out of Greek resources in the history of Greek. Chadwick considers *o-po-qo* 'blinkers', but makes no connection with any Greek words.

o-pi-i-ja-pi is even less securely identified than the other words. It is translated 'bit' by Ventris and Chadwick (*Docs*:365), but there is no very convincing internal reason for this translation, nor are the Indo-European connections of a sort to inspire confidence, though of course none of them is excluded a priori or impossible. Again Homer has a different word for this, *khalinós* (*Iliad* 19.393), but here there may be a good reason for the divergence in terminology between Mycenaean and Homer. Bits were made of bone in the Mycenaean period (as are these—*ke-ra-ja-pi*) and at the same time in the Danubian region (*Docs*:365, Lorimer 1950:308). Bronze bits are known from contemporary Greece, but are apparently rare. It is at least possible that the word *khalinós* is connected with *khalkós*, and specifically denotes 'bronze bit'.

i-qo-e-qe has been identified as the pole-stay characteristic of Mycenaean chariots, and illustrated on the ideograms *240 and *241, though not on *242 (*Docs*:364). The only good reason for this identification seems to be that the object is *do-we-jo* (= *dorweios* 'wooden'), and there is little else that might be so described. Morpurgo (1963:115) has preferred to leave the word untranslated. In any event it seems somehow to be connected with *i-qo*.

Other tablets and series of tablets serve to increase our knowledge of the

Mycenaean chariot terminology. PY Sb1315, though problematic in many points, clearly deals with objects made of leather, among which is included *a-pu-ke* 'horse's headband', the classical *ámpux*. Indo-European connections are present here, for *ámpux* is a compound of *am* = *aná* and *-pux*, related to the Avestan *pusā* 'diadem', and derivable from an Indo-European **pukā* (Frisk *GEW* 1.96). But this connection is not sufficiently specific to chariots to prove an Indo-European origin of this part of the harness arrangements.

Ub1318, also dealing generally with leather objects, apparently specifies that one such object is destined for *ze-u-ke-si*. It seems likely that these *ze-u-ke-si*, whatever the explanation of the form, have something to do with chariots, and hence provide evidence, slight though it be, for the word *zugón* in Mycenaean.

The word for 'wheel' in Mycenaean, or perhaps the word for 'carriage' is *a-mo* (= [*h*]*armo*), the later *hárma* 'chariot'. Again Indo-European connections are present, though not impressive. *a-mo* can be connected with the verb *ararískō* though there are difficulties with the spiritus asper. These difficulties are not sufficient, however, to indicate Anatolian provenience for the word (cf. Frisk *GEW* 1.143).

PY Vn10 reads:

o-di-do-si du-ru-to-mo
a-mo-te-jo-na-de e-pi-pu-ta 50
a-ko-so-ne [[100]] 50
to-sa-de ro-u-si-jo a-ko-ro a-ko-so-ne
100 to-sa-de e-pi-pu-ta 100

The context clearly has to do with wood (*du-ru-to-mo* = *drutómoi* 'wood-cutters'), and this wood is to be sent to the shop or factory that manufactures wheels (*a-mo-te-jo-na-de*). Hence it is not at all unreasonable to identify *a-ko-so-ne* with *áxones* 'axles', a word with an excellent Indo-European pedigree (Frisk *GEW* 1.116).

The Mycenaean word for chariot in general seems to occur on a series of wheel tablets from Pylos, e.g. Sa834: *a-me-ja-to wo-ka we-je-ke-e* ROTA + TE ZE 1 *'we-je-ke-e* wheels for the chariot of *a-ma-ja.'* It is clear that *wo-ka* is in some way connected with the classical *ókhos* (Chadwick-Baumbach 1963:231), and hence with the Indo-European **wegh-*. The Mycenaean form seems to represent a **wokhā*.

We do not have all the words connected with chariots known to the Mycenaeans, but we do have enough to show that words denoting the major parts of chariots are of Indo-European derivation: the chariot itself, the wheels, the axle, the yoke, the reins, as well as possibly the horse's head-band. Details of the harness, however, apparently were not Indo-European, or if Indo-European, were calques on foreign words, or new formations created by means of Greek resources, for new or different objects. Few of them are used for the same objects later on.

Homer provides us with much fuller information about the chariot.[11] Two passages are of supreme importance here. *Iliad* 5.722–730 reads:

> Ἥβη δ' ἀμφ' ὀχέεσσι θοῶς βάλε καμπύλα κύκλα,
> χάλκεα ὀκτάκνημα, σιδηρέῳ ἄξονι ἀμφίς.
> τῶν ἧτοι χρυσέη ἴτυς ἄφθιτος, αὐτὰρ ὕπερθε
> χάλκε' ἐπίσσωτρα προσαρηρότα, θαῦμα ἰδέσθαι·
> πλῆμναι δ' ἀργύρου εἰσὶ περίδρομοι ἀμφοτέρωθεν·
> δίφρος δὲ χρυσέοισι καὶ ἀργυρέοισιν ἱμᾶσιν
> ἐντέταται, δοιαὶ δὲ περίδρομοι ἄντυγές εἰσι.
> τοῦ δ' ἐξ ἀργύρεος ῥυμὸς πέλεν· αὐτὰρ ἐπ' ἄκρῳ
> δῆσε χρύσειον καλὸν ζυγόν, ἐν δὲ λέπαδνα

There is no doubt about the meaning of any of these words, and it is therefore necessary only to list the relevant terms with their meanings, and to indicate the Indo-European cognates if there are such.

ókhea	'carriage'	< **woghos*, IE **wegh-* (Frisk *GEW* 2.257–258)
kúkla	'wheels'	< **kʷukʷla*, IE **kʷekʷla* (Frisk *GEW* 2.44–5)
knḗmē	'spoke'	< **kna:ma:*, IE **kna:m-*, cf. OIr *cnāim* 'bone' (Frisk *GEW* 1.883)
áxōn	'axle'	< **aks-*, cf. Skt. *akṣa-*, L. *axis* (Frisk *GEW* 1.116)
ítus	'felloe'	< **witus*, IE **wit-* 'beigen, flechten' (Frisk *GEW* 1.743–744)
epíssōtra	'tires'	< *epi* & *sō̂tron* (Pollux). Frisk (*GEW* 1.542) connects this word with Skt. *cyautná-* 'Unternehmung, Tat' Ave. *šyaōlna* 'Tun, Handeln', and hence supposes an IE **kʷyō[u]-t(e)r-*, *-t(e)n-*. Such a connection is of course possible, but not inevitable.
plḗmnē	'nave'	Generally connected with *píplēmi*, on the assumption of a meaning 'die Fülle des Rades' (Frisk *GEW* 2.560), and this interpretation has been generally accepted, though Chantraine (*Formation des noms en grec ancien* [Paris, 1933], p. 215) is dubious on semantic grounds.
díphros	'chariot-board'	is generally, and probably correctly, taken to mean 'zwei-Träger', and analyzed as **di-phr-os* IE **di-* 'two' and **pher-* 'carry' (Frisk *GEW* I.400–401).
ántux	'rail'	This was the rail around the front edge of the chariot, and is a compound of *ana* and *-tux*, a root-noun to the verb *teúkhō* (Frisk *GEW* 1.115), a verb exampled also in Mycenaean,

which can easily be derived from an IE **dheugh-*. Cognates for this root are not frequent or impressive, but do exist.

rhumós 'pole' A derivative of the root **weru* ~ **wrū-* seen in *erúō* 'draw, drag', but without certain extra-Greek connections (Frisk *GEW* 1.571).

zugón 'yoke' IE **yugóm* 'yoke' (Frisk *GEW* 1.615–616).

lépadna This was the broad leather strap (or straps) which fastened the yoke to the neck of the horses, and which was connected also with the girth. In Homer the word may have denoted both the neck strap and the girth. There is no IE etymology (Frisk *GEW* 2.104–105), and the formation indicates non-Greek origin.

Of the above, the words for 'chariot', 'wheels', 'axle', and 'yoke' are certainly IE words, and probably were part of an IE technical vocabulary: only they can be considered diagnostic. 'Chariot-board', 'felloe', and 'rail' may be new formations not of IE date; and 'spoke' is a metaphorical use of the word for 'leg'. 'Tire', 'nave', and 'pole' are uncertain; 'strap' definitely borrowed.[12]

Though a fair number of IE terms were found in connection with the two-wheeled battle chariot just discussed, there are far fewer connected with the four-wheel mule wagon described in *Iliad* 24.266–75. Of course part of the reason for this is that the passage in book 24 is more technical than the other, and also describes more detailed pieces, items which are apt to be derived from a special trade jargon, and not from the general stock of Greek words. The interpretation of some of them is disputed.

ἐκ μὲν ἅμαξαν ἄειραν ἐύτροχον ἡμιονείην
καλὴν πρωτοπαγέα, πείρινθα δέ δῆσαν ἐπ' αὐτῆς,
κὰδ δ' ἀπὸ πασσαλόφι ζυγὸν ἧρεον ἡμιόνειον
πύξινον ὀμφαλόεν, εὖ οἰήκεσσιν ἀρηρός·
ἐκ δ' ἔφερον ζυγόδεσμον ἅμα ζυγῷ ἐννεάπηχυ·
καὶ τὸ μὲν εὖ κατέθηκαν ἐυξέστῳ ἐπὶ ῥυμῷ,
πέζῃ ἔπι πρώτῃ, ἐπὶ δὲ κρίκον ἔστορι βάλλον,
τρὶς δ' ἑκάτερθεν ἔδησαν ἐπ' ὀμφαλόν, αὐτὰρ ἔπειτα
ἐξείης κατέδησαν, ὑπὸ γλωχῖνα δ' ἑκαμψαν.
ἐκ θαλάμου δὲ φέροντες ἐυξέστης ἐπ' ἀπήνης

ámaxa 'chassis of four-wheeled wagon'. Frisk (*GEW* 1.86) seems to subscribe to the view that this is a derivative formed from *háma* and *áxōn* used as an adjective modifying *kúkla*. Thence it became singular, and was transferred to the wagon. This is a rather involved and unlikely development, but if correct, provides an IE etymology, though not a diagnostic one, since it is a compound, and may represent a calque or simply a new

formation. It seems to me most likely that the word is borrowed.

hēmioneíē 'mule-drawn'. Homer is insistent on the fact that this is a mule-drawn cart, and further that it receives a mule-yoke. Apparently there was some other kind of cart, and we may assume another kind of yoke, this probably involving horses. The word is either a compound consisting of *hēmi-* 'half' and *ónos* 'ass', or is a corruption of a non-Greek word sufficiently like *hēmíonos* to be reinterpreted as such. Whatever the case, neither *hēmíonos* nor *ónos* are IE words, and were doubtless acquired from a Near-eastern or Pontic source together with knowledge of the animals themselves.[13]

peírins 'wicker basket tied on the *ámaxa*'. If only because of the suffix we can assign non-Greek origin to this term. The Greek translation of this is *huperteríē* (*Odyssey* 6.70).

omphalós 'button or knob in the middle of the yoke'. Clearly a technical application of an originally nontechnical IE term (cf. Frisk *GEW* 2.391–2).

oíēkes 'rings of the yoke' (?) through which the reins were passed. IE cognates seem assured by the Finnish *aisa* 'Stange der Gabeldeichsel' borrowed from Baltic **aisō* or **aisa-* < IE **oisā-*, **oiso-*, forms which must lie directly behind the Greek forms. Somewhat remoter cognates are seen in Sanskrit *īṣa-* 'Deichsel' and Hittite *ḫišša* 'id'. (Frisk *GEW* 2.356).

zugódesmon 'yoke-band'. Clearly a compound of *zugón* and *déō*, but again clearly nondiagnostic.

pézē The far end of the pole (?). Though the interpretation is uncertain, clearly the word is IE: < **pedyā*.

kríkos 'ring'. The function of this object is not certainly known, but the word has no secure IE etymology anyway, and its range of application is not restricted to wagons.

héstōr Interpretation uncertain, and though it has a plausible IE appearance, it lacks a plausible IE etymology.

glōkhís 'end of the yoke-strap'. This represents a special use of the word for 'tip' of non-IE origin.

apḗnē This is the four-wheeled wagon assembled, with both body and chassis. Again the origin of the word is unknown (Frisk *GEW* 1.121).

From the fact that so few words concerned with the harness are of IE origin, we may with some show of reason infer that intricate yoke-arrangements of the sort described in *Iliad* 24 were not known to the speakers of IE who later became Greeks. And we may presume also that the same type of yoke arrangement or a similar one was used on two-wheeled battle-chariots. Furthermore several words (*zugón*, *rhumós*) are common to both types of vehicles. But several items concerned with the wagon itself, as opposed to

the yoke and harness, are of non-IE origin, or at best are new words formed of inherited material within the history of Greek.

In Homer both *ókhea* and *hárma* refer to the chariot in general and are synonymous, but Linear B seems to indicate that originally the terms differed, and one denoted the unassembled chariot, the other the assembled.[14] Both words are IE, however, while the corresponding terminology for the four-wheeled wagon shows *ámaxa* for the unassembled wagon, *apḗnē* for the assembled, neither one an inherited IE term. And the cab of the chariot is called *díphros*, clearly Greek, though not clearly of IE date, while the body of the wagon was called the obviously non-IE *peírins*. In all these cases the chariot utilizes IE terminology, the wagon innovating. What historical conclusions are we to draw from this discrepancy in terminology? One conclusion, an obvious one, is that the vehicles were differently put together and served different purposes. Speed would have been a consideration for the one, strength and durability for the other. It is most likely that spoke wheels (*kúkla*, as opposed to *trokhoí*?; cf. *eútrokhon* in *Iliad* 24.266) were used only on the two-wheeled chariot, while the wagon used solid wheels. In the earlier version of this paper read in Philadelphia I drew the further conclusion that, since the terminology describing the four-wheeled wagon was clearly non-IE, the IE invaders knew the two-wheeled chariot but not the four-wheeled wagon, but this now seems most unlikely. Both Cowgill and Hamp have since pointed out to me that the IE vocabulary descriptive of wheeled vehicles inherited by the Greeks is not sufficiently specific and restricted to two-wheeled chariots to the exclusion of four-wheeled wagons to allow such a conclusion. That wagons similar to Priam's (*Iliad* 24) existed in IE times seems probable, though not proved linguistically, but, as we have seen, they cannot have been mule-drawn: presumably they would have been pulled by oxen.

But since the vocabulary descriptive of wheeled vehicles is so nonspecific, is it even possible to conclude that the IE invaders of Greece, whether or not they know of the more homely four-wheeled wagon, at least knew of the two-wheeled battle chariot? Crossland (1957:22–3), without committing himself, mentions that some archaeologists doubt whether the IE peoples possessed the chariot: 'There is no inherited word for it, or for any part of it which a cart would not have, and there is no philological proof that any Indo-Europeans had it before c. 1450 B.C. . . . ' If the IE peoples had no chariot, then clearly my argument concerning Indo-Europeanization and the introduction of the chariot is destroyed. But it really does seem that, though there is no one word, since all Greek words dealing with the chariot are of IE origin, while none dealing with the wagon are, that the one reflects terminology inherited from the time of the incursion of IE peoples, the other innovating terminology probably borrowed from the local inhabitants.[15] This impression is strengthened when we recall the fact signaled by Porzig (1934:90–1) that there is a distinction between the two-wheeled battle-wagon and the cart also in Latin (*currus-plaustrum*), though here the words are not inherited; and in Sanskrit (*ratha-anas*), where both are in

herited. The lack of a single IE word can in this case be plausibly explained by assuming either that the chariot, being a recent invention, was still called simply 'wheels' (vel sim.); or that, as in the case of our own vehicle terminology, several different varieties of the chariot had developed, and different language groups continued different inherited terms.[16]

We may therefore conclude that the IE invaders of Greece knew the chariot when they first entered Greece, and may assume that they arrived with or on them. They doubtless knew, but probably did not bring with them, their four-wheeled wagons. When we attempt to join this linguistic conclusion to the archaeological evidence, we find that chariots are found in Greece first in connection with grave circle A at Mycenae ca. 1600. From this we may draw the conclusion that IE speech, hence the Greeks, arrived at about that time.

This conclusion is buttressed to a certain extent by the word for 'horse' *híppos*, of obvious, but difficult, IE origin (Frisk *GEW* 1.733–35). The Greeks cannot have entered Greece before they had become acquainted with the horse, though they need not have entered at the same time. Here the archaeological remains are not so satisfactory, but it is nonetheless true, so far as I know, that horse bones have been found in MH Greece only at Malthi, in a context difficult to date, and that therefore it is certainly possible, if not plausible, that the horse and chariot entered Greece at the same time. Of course the horse was known already in the earliest stages of Troy VI in the nineteenth century, and elsewhere in this volume Professor Thomas mentions even earlier examples in the Mediterranean area, so this particular item of proof is not so impressive as is the chariot.

But it is not usual to assume that Greece was Indo-Europeanized as late as 1600. Classical archaeologists favor 1900, and prehistorians such as Thomas and Gimbutas support a date of ca. 2300–2200. Thus there is a maximum discrepancy of 700 years between my conclusion and the conclusion of classical archaeologists and prehistorians. Perhaps this is inevitable given the fact that we operate with different sorts of data, but it is too bad. I can unfortunately see no way to bridge the gap, but might here emphasize the provisional and essentially negative nature of my conclusion. I base my conclusion on what I presume to be linguistic fact coupled with the presently attained results of Bronze Age archaeology in Greece. More specifically, my conclusion is inextricably bound up with the chariot: if a chariot, or evidence for a chariot, is found in Greece dating from before 1600, then my argument will lead me to assume that the Greeks can have arrived at that earlier date, though they need not have: my terminus post quem will move back. But until that time I will continue to hold that we have no evidence for Indo-European speech in Greece prior to ca. 1600.[17]

Notes

[1] The fullest recent critical treatment of the Pelasgian question is to be found in D. A. Hester, 'Pelasgian'—a new Indo-European language? *Lingua* 13(1965)335–84.

[2] We know neither where nor when, through at present the strongest claimant

archaeologically is the third millennium 'Kurgan' culture. But we do know at least that IE speech did not originate in Greece, and that therefore the question of the arrival of the Greeks is a legitimate one. That Greek is not indigenous, and that all other IE languages are not the result of migrations from Greece, is indicated among other things by the fact that bronze, known in Greece from ca. 3000 B.C., is called *khalkós* only there, and not in any other IE language.

[3] I owe the if-clause to Eric Hamp, who objected (correctly) to the implication that Linear B = proto-Greek, which it manifestly does not. Most scholars in fact feel that in Mycenaean times (ca. 1200) there already existed three, if not all four, of the classically attested Greek dialects (cf. W. Cowgill, Ancient Greek Dialectology in the Light of Mycenaean in H. Birnbaum, and J. Puhvel, *Ancient Indo-European Dialects* (Berkeley & Los Angeles, 1966) 77–95, (particularly 93). We therefore must imagine a rather rapid split into dialects, perhaps as yet little differentiated, between ca. 1500 and ca. 1200.

[4] Cowgill (per lit.) expresses skepticism concerning the equation: horse & chariot = new people: 'Considering the rapid diffusion of horses in post-Columbian North America, and in general the eagerness with which people might be expected to adopt such a desirable animal, I would not think the appearance of horses and chariots very cogent evidence for a new people.' I agree, and shall later regard the horse and chariot as merely a terminus post quem. Incidentally, Stubbings (1964:21) feels that horses may have been imported from Troy.

[5] Hamp (per lit.) informs me of the interesting case of Navaho which provides still another possibility. Apparently the Navaho consistently named all the parts of the automobile completely in Navaho, making calques on words for parts of the body. In this they have done more consistently what the Greeks did when they called a spoke *knēmē* 'leg'. I should guess that the Navaho situation is unusual in linguistic history.

[6] This can be stated still more negatively. Meaningless phonological sequences are not borrowed; only words are borrowed. The only partial exception known to me is the case of my son who, at the age of two and a half, was taught to say the French *u*. He learned the sound, but said it only on demand, since it had no referent, and was simply a sound. Finally he found a small plastic representation of the letter *e*, and this he called [y]. He had discovered the previously unknown referent.

[7] Perhaps the most familiar instance of this type of behavior is the English settlers' use of robin to denote a large thrush in this country because the smaller, warblerlike, thrush which they had previously designated by that name is not found here.

[8] In a standard case. But we should perhaps say, to be on the safe side, that they cannot have preceded knowledge of the innovation. I suspect, for instance, that knowledge of the elephant preceded the beast to these shores by several centuries. Knowledge of a church building, to use an example drawn from more ordinary circumstances, preceded churches to this country also, though not by much.

[9] To assume two waves of IE speakers, one arriving without the sheep ca. 2100, but finding the sheep there; and another ca. 1900 bringing both the sheep and */ówis/, is possible, but vitiates the problem. For surely part of the definition of IE involves IE being that language which had a sequence /ówis/ meaning 'sheep'.

[10] Hamp (per lit.) remarks that Sanskrit *nāsyam* looks very much like a reinterpretation of **ansia* by folk-etymology. And so it does, but since a bridle always in fact involves what might be considered the horse's nose, perhaps the original form of the word was precisely **nā̆syā-*, and it experienced metathesis (for some unexplained reason) in the common ancestor of Greek and Irish. If this should turn out to be a plausible interpretation of the facts, we should have strongly confirmatory evidence that the IE peoples had a chariot, or at least an animal-drawn vehicle requiring a bit arrangement. For otherwise we could state at best that various IE languages called portions of a horse's bridle arrangements 'nose-piece', a formation which might well be created independently in various languages. The metathesis in both Celtic and Greek excludes this possibility.

[11] Bibliography on the Homeric chariot: *Docs*:361–64, Wace and Stubbings 1963: 521–22, 539–41; Lorimer 1950:307–28; W. Leaf, *The Iliad*, 2nd ed., II (London 1902, reprinted Amsterdam, 1960) 623–29; Daremberg, C. and Saglio, E. *Dictionnaire des antiquités*, I, pt. 2 (Paris, 1887, reprinted Graz, 1962) 1633–43.

[12] The etymology of 'tire' is uncertain, but since the *epíssōtron* is clearly on something, and since the word *sôtron* is not exampled until Pollux, and hence possibly an abstraction from the Homeric compound, it would seem indicated to connect *epíssōtron* with *ítus* etymologically as well as physically. We might then suppose an original **epi-witu-ōtron* which, with haplology, would become **epitwōtron* > *epíssōtron*. Or possibly we can assume a late development with loss of -/w/ -:/*[epiwitwōtron] > *[epiissōtron] = [epīssōtron]. The *ō* is, however, unexplained on this hypothesis.

[13] I confess that I am troubled by the etymology of *hēmíonos*, 'half-ass', quite apart from the pun of relatively recent origin, seems a strange formation. Why not call the beast *hḗmippos*? And why 'half' of anything? Compounds with *hēmi-* generally signify that the object in question is the result of splitting a whole in two: two of them would, if put together, make a whole, as in the case of *hēmitálanton* 'half a talent', *hēmimédimnon* 'a half-*médimnos*'. There are exceptions of course such as *hēmibárbaros* 'half-barbarous' (Strabo 13.1.58) and *hēmídoulos* 'half-slave' (Euripides *Andromache* 942), but such formations are either poetic or late. The only early parallel is *hēmítheos* 'half-god' in *Iliad* 12.23, but this word occurs only there in Homer, is difficult of interpretation, and for various reasons suspect (W. Leaf, *The Iliad* I [2nd ed., London, 1900, reprinted Amsterdam, 1960] 527). The word occurs later in more understandable context in Hesiod (*Opera* 159). *hēmíonos* is thus isolated. What is worse, the adjectival derivative of this word is *hēmióneios*, not the expected **hēmiónios*. This can be excused by the metrical requirements of Homeric verse, but the later-attested forms *óneios* 'of an ass' (Aristophanes *Equites* 1399, Aristotle *Historia Animalium* 522[a]28) cannot, and we are surprised to find a derivative of an *o*-stem in *-eios*. To be sure there are analogous forms, such as *híppeios*, *aígeios*, etc., but these may also be poetic creations, and at least in the case of *híppos*, *híppeios* is later generally discarded in favor of *híppios*. Thus *óneios* may point to a neuter noun **ónos*, **óneos*, of which *óneios* would be a perfectly regular derivative. Such a noun inevitably reminds us of Latin *onus* 'load, burden' and Sanskrit *anaḥ* 'wagon', both deriving from an earlier **onos*. If this etymological connection is correct, *hēmíonos* would have in origin been a determinative compound meaning '*x* a wagon/load' where *x* could be either a verb or an adjective, most likely a verb, with a first element either borrowed, or connected with Latin *emō* 'take' and possibly with Greek *énegka* < **ememka* (?), the whole meaning 'load-taker, load-carrier, wagon-drawer', which, when in the phrase **hēmíonos híppos* (cf. *equus onager*) came to be taken as an *o*-stem adjective modifying *híppos*, and a new declension developed. Originally *hēmíonos* applied to both the ass and the mule. But it then became restricted to the mule when by popular etymology the word *ónos* 'ass' was abstracted from the compound. Thus, though perhaps not borrowed, *hēmíonos* is at least of secondary formation, not of IE date.

[14] This is of course uncertain, but since *a-mo* means 'wheel' or 'carriage', we may assume that it developed from 'carriage' → 'wheeled chariot' → 'chariot (in general)'; while *wo-ka* always is wheelless (on PY Sa-tablets), and hence apparently had the meaning 'chariot (in general)', but when opposed to *a-mo* meant specifically 'wheelless chariot'. *i-qi-ja* must have been the generic term for the two-wheeled chariot.

[15] Or, since the IE invaders were presumably of a ruling, warrior class, perhaps these words were imported into the language by those who used them, that is to say, the pre-Greek inhabitants. As these people learned Greek, they found that no Greek word suited their four-wheeled wagons, and hence imported their own terms into their newly acquired language, whence they spread to the speech of other Greeks. Thus they gave up all the words of their own language for which they could find IE equivalents, retaining only those for which there were no equivalents.

[16] If we should for the sake of argument assume that the IE peoples had only four-wheeled wagons, we should have to imagine the following development or one like it.

(1) The inherited IE terminology applied only to four-wheeled vehicles, but was extended to include the two-wheeled chariot when the IE peoples, among them the Greeks, came upon the chariot, or when it was introduced to them (ca. 1600). (2) At that time the Greeks applied the same terminology to two-wheeled chariots and to four-wheeled wagons. (3) But then they restricted the inherited terminology to the innovation, the two-wheeled chariot, and introduced borrowed terms for the four-wheeled wagon, their original vehicle. Such a development is not, I suppose, impossible, but is perhaps less likely than the assumption advocated in the text, an assumption which avoids stage (1) of this scheme, and places stage (2) as the original.

[17] I wrote this paper while a postdoctoral fellow at the Institute for Research in the Humanities, University of Wisconsin. I am much indebted to the Institute and its director, Kenneth Setton, for providing me with a year free from teaching and pleasant surroundings in which to work.

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Italic and Celtic Superlatives and the Dialects of Indo-European

Warren Cowgill

At the Conference on Indo-European Linguistics held in Los Angeles in 1963 the question of a special Italo-Celtic subgroup of Indo-European was very ably discussed by Calvert Watkins.* He concluded (Watkins 1966:43–4) 'that Italo-Celtic unity is a myth. The only common language from which both Italic and Celtic can be derived is Indo-European itself; the only single grammar that could account for each would be the grammar of Indo-European.'

With this formulation I agree, as far as phonology is concerned. The one exclusively shared sound change of Italic and Celtic is the assimilation of *p to a k^w farther along in the same word, as in OIr. *cóic*, Lat. *quīnque* '5' from **penk^{w}e*. The paucity of examples is not a good reason to doubt that this is a real sound law. If there happen to be only two or three examples of a particular sound combination in the protolanguage, and all develop in the same phonetically plausible way, the scholar has no choice but to accept that development as lautgesetzlich—with, of course, much more reserve than if there were more examples. But, as Watkins says (1966:33), the differing outcomes of **perk^{w}u-* 'oak' in Latin and Celtic indicate that when the assimilation occurred Italic and Celtic were already differentiated by a change of **k^{w}u* to *ku* in Celtic. To be sure, this is a trivial difference, less than some of the differences that probably existed between Indic and Iranian prior to some of the diagnostic Indo-Iranian shared innovations. More important, perhaps, is the triviality, pointed out by Hoenigswald (1966:7n16), of the change *$p \ldots k^w > k^w \ldots k^w$ itself, which resulted in no homonymy and only, as it were, simplified slightly the rules for the cooccurrence of the features 'labial' and 'velar' in connection with the features 'stop' and 'voiceless'.[1]

But I think that in the area of morphology Celtic and Italic do share some deep-cutting innovations which not only are exclusive to them, but also cannot be shown to be later than innovations found in one but not the other (whether recurring in some other branch of Indo-European or not), just as their one shared phonologic innovation cannot be shown to be later than any significant divergences.

But these shared morphologic innovations are all very early, belonging to what might be called 'late Proto-Indo-European', a language that was

phonologically essentially unchanged from the 'Proto-Indo-European' that we can reconstruct by straight comparison without recourse to internal reconstruction and that morphologically as well was very little developed beyond that stage. These shared innovations are not very numerous, and suggest a rather short period of common development followed by a long period of divergence prior to our oldest documents, during which they are overlaid by new innovations different in each subgroup, so that Italic and Celtic might be said to constitute a 'drowned' or 'prematurely disrupted' subgroup of Indo-European, in contrast to the well-recognized double-jointed subgroups, Balto-Slavic and Indo-Iranian, where the period of common development was long enough and close enough to the earliest documents that its existence is unmistakable. It might be said that in reopening the grave just closed by Watkins I have found neither an empty coffin nor a living adult prematurely buried, but a stillborn infant. Whether in this situation we find a term Italo-Celtic useful or not depends mainly on the period about which we are talking. Already before the Celtic invasion of Italy around 400 B.C. Italic and Celtic were different enough to warrant being treated as two distinct subgroups. But in discussions of the 'breakup' of the ideal, mythical, totally homogeneous Indo-European proto-language into recognizable dialects, we would be justified in dealing with a single 'Italo-Celtic' entity in the far west of the Indo-European area that only later—say very roughly 2000 B.C. ± 500 years—became split into two, rather than with two distinct areas from the beginning, each sharing some features with one or more of its other neighbors.[2]

What has led me to a different interpretation from Watkins is the sorting out of Italic and Celtic agreements and divergences into a relative chronology. Watkins (1966:passim) quite rightly insists that for each of the well-known and oft-repeated Italo-Celtic agreements there are considerable differences in the details as they appear in the historically attested Italic and Celtic languages. But he has not said much about the relative chronology: are the divergences as old as the agreements, or even older, or can they be shown to be later, the result of over a millennium of independent changes? In several cases I think the divergences can be shown to be later than the shared innovation. In what follows I intend to demonstrate this in some detail for the superlatives and more briefly for some other innovations, and then put forth some tentative remarks about the implications for prehistoric population shifts in central Europe.

1. Let me begin by rehearsing what is already well known about the formation of comparatives and superlatives in Indo-European. Proto-Indo-European did not have regular morphologic categories 'comparative' and 'superlative', at least not in the earliest stage we can reconstruct. It did have two suffixes which appear as comparative formants in the attested Indo-European languages. One was an athematic ablauting suffix of archaic type added direct to roots, *-yŏs-*, *-yes-*, *-is-*, meaning something like 'possessing the quality designated by the root to a more than average degree'. Thus to the root **yewH-* '(be) young' there not only was an *on*-derivative

**yuH-on-* 'young' with no overtone of excessive youth or of contrast with anything other than 'old', but also a *yos*-derivative **yéwH-yos-* meaning roughly 'rather young, quite young', with an implication that the creature so qualified was young in comparison to some standard of youth, expressed or implied. For such a formation to become a regular comparative it is necessary first that it lose the restriction on its occurrence to root formations and become capable of occurring with derived adjectives, and second that the syntax change, making it necessary to use such *yos*-derivatives instead of the usual adjective whenever two entities are being explicitly compared. This happened to *-yos-* in both Italic and Celtic, leading to the regular comparative suffixes Lat. *-ior* (cf. Osc. *ma-is* 'more'), OIr. *-(i)u*, and British relics like M(iddle)W(elsh) *llei* 'smaller', *hyn* 'older'. (The regular British suffixes, W *-ach*, Co(rnish) *-a*, *-e*, Br(eton) *-oc'h*, are a still later development, of course.)

2. But making *-yos-* the regular comparative formant is not an exclusively shared Italo-Celtic innovation, since it occurs also in Germanic and Balto-Slavic.

3. The other suffix that became used to form comparatives was *-tero-*, which occasionally appears without *-t-*, as *-ero-*.[3] This was a secondary suffix, added not to roots but to pronominal and adverbial stems, and serving to emphasize a CONTRAST between TWO items. Thus, added to the pronoun **k^{w}o-* 'which (of an indefinite number)?' it formed **k^{w}o-tero-* 'which of the two?'; added to the adverb[4] **en* 'in' it made **en-tero-* 'the inner one' (opposed not to a less inner entity but to 'the outside one'). In this function it is preserved, at least as a relic, in all branches of IE except perhaps Tocharian;[5] only in Greek and Indo-Iranian was it promoted to a regular comparative formant.[6]

4. Italic and Celtic both continue to use *-(t)ero-* essentially with the PIE meaning of contrast and limitation of occurrence to pronominal and adverbial stems; there is no common innovation here. True, both use the suffix to form possessive adjectives to the nonsingular personal pronouns, but this feature is shared with Germanic, Greek, and Armenian, and the implementation differs in Italic and Irish (the divergent British forms are probably a later development). Lat. *nos-tro-*, *vos-tro-* (Cl. Lat., Umbr. *ves-tro-*) are built on the PIE enclitic oblique plurals **nos*, **wos*, while OIr. *náthar* 'of us two' and *fāthar* 'of you two', whence the regular proclitics *arN* 'our' and *farN*, *barN* 'your',[7] are built on the enclitic duals **nō*, **wō*: and the hapax *sethar-si* 'uestram', Wb 1b2, shows that in Celtic the plurals were built on the accented obliques **n̥smé*, **usmé*, just like Gk. *hēmé-teros*, *humé-teros*.[8]

5. In the formation of the superlative, if we leave Italic and Celtic aside for a moment, we find that the two comparative suffixes, *-yos-* and *-(t)ero-*, each has its own associated superlative formation. To be sure, the material is limited to Germanic, Greek, and Indo-Iranian. The remaining subgroups —Anatolian, Armenian, Tocharian, Balto-Slavic, Albanian—do not have morphologic superlatives and apparently lack even relics of such forms.[9]

This of course does not mean that they necessarily never had them, only that they represent gaps in any attempt to reconstruct the prehistory of the Indo-European superlative formations.[10]

6. Paired with the contrastive *-(t)ero-* we find superlatives in *-(m̥)mo-*, *-tm̥mo-*. Indo-Iranian comparatives in *-tara-* regularly have beside them superlatives in *-tama-*. In Greek the suffix acquired a second *-t-*, resulting in *-tato-* beside *-tero-* and *-ato-* beside *-ero-* (a relic of the unextended form occurs in *prómos* 'front-rank man, chief'). In Germanic, where *-(t)ero-* did not expand beyond its PIE bounds, *-mo-* and *-(t)m̥mo-* are similarly limited to adverbial bases and one or two other words indicating position, e.g. Go. *innuma* 'inner', *hleiduma* 'left', OE *forma* 'first'.[11]

7. In Italic and Celtic *-(t)m̥mo-* is similarly limited in extension. I list below all the clear and probable examples of it. Where related forms in *-(t)ero-* are attested I have cited them, since they are prima facie evidence that we are dealing with *-(t)m̥mo-* and not some other suffix.

8. With *-(m̥)mo-* are formed the Latin pair *summus* 'highest' < **sup-(m̥)mo-* and *īnfimus* 'lowest' < **n̥dh-m̥mo-*, beside the contrastives *superus* 'upper', *īnferus* 'lower'. The first of these recurs in U *somo*; and Celtiberian *ueramos* appears to be a replacement of a corresponding Celtic **u(p)amo-*, brought about by the fact that **u(p)amo-* agreed in meaning with **u(p)er* 'super', not with **u(p)o* 'sub'. As Tovar kindly informs me, the original **upm̥mo-* may be preserved in the place name *Vama*, which Ptolemy 2.4.11 lists among the towns of the Celts of Baetica. Here also belongs U *promom* 'first', derived, like Gk. *prómos*, from the adverb **pro* 'forward, in front', and probably the Latin adverb *dēmum* 'at last, finally', from the adverb *dē* 'down'.[12]

9. On the border between *-m̥mo-* and *-tm̥mo-* are Lat. *postumus*, O **pustm**[, *posmom* 'last' beside Lat. *posterus*, O **pústreí**, U *postra* 'later'. Descriptively these are derived from *post* 'later' with *-m̥mo-*. But *post* appears to be built on an earlier **pos*, and if *postumus* etc. were formed while **pos* still existed, they have suffixal *-t-* (Sommer 1900:10).

10. With *-tm̥mo-*, *-tero-* are formed three pairs of antonyms in Latin: *extimus* 'outmost' (*exterus* 'outsider') and *intimus* 'inmost' (**intero-* 'inner' in *interior*, *intrā* etc.); *citimus* 'closest' (*citer* 'closer' in Cato, usually *citerior*, *citrā* etc.) and *ultimus* 'farthest' (*ultero-* 'farther' in *ulterior*, *ultrā*, etc.); *dextimus* 'farthest to the right' (*dexter* 'right') and *sinistimus* 'farthest to the left' (*sinister* 'left'). *Optimus* 'best' is probably also an example, despite the lack of a relative in *-tero-* and the divergent meaning of its base adverb *ob* 'against, in return for, on account of'. But *ob*, cognate with Gk. *epí* 'on', *ópi-sthe* 'in back', Hitt. *appa* 'zurück', was probably originally a case form of a noun meaning 'back, upper surface', so that **op(i)-tm̥mo-* could well mean 'topmost' and from that develop into 'best'.[13] I am not sure that the augural term *tripudium sol(l)istimum* for the favorable omen consisting of bits of food dropping from the sacred chickens' mouths contains a superlative; if it does, Sommer (1914:457) is probably right in seeing it as a secondary formation modeled on *sinistimus*, also used in the augural language.

P-Italic and Celtic have fewer examples of *-tm̥mo-*. Oscan provides **últiumam** 'ultimam', and Umbrian *hondomu* 'from the lowest', *hondra* 'infra', for which O **húntrú-** 'lower' attests an originally voiceless dental.[14] From Celtic Pedersen (1913:123) cites W *eithaf* 'farthest' (: *extimus*), and *gwarthaf* 'top, summit' < **wor-tamo-*—this last evidently a replacement or rival of the *wer-amo-* mentioned above in § 8. In continental Celtic, where *wer* 'over' was not changed to **wor* under the influence of its antonym *wo*, Fleuriot (1964:251) mentions a Gaulish personal name *Vertamica*, and Pliny mentions a tribe *Vertamocori*. A third Celtic example of *-tm̥mo-* is in OW *cisemic* 'first', if the explanation given in § 24 is correct.[15]

11. Just as in § 4 we saw that Italic and Celtic shared no significant innovation in the use of PIE *-(t)ero-*, so we see now that they share no significant innovation in the use of *-(t)m̥mo-*.

12. But before leaving this suffix there is something to say about its origin. Probably the original form was just *-(m̥)mo-*, and *-tm̥mo-* arose by analogic proportions of the type *(s)up-ero-* : *(s)up-m̥mo-* = *en-tero-* : X. But where did *-(m̥)mo-* come from? It seems to be universally accepted that it is from the PIE ordinals **sept(m̥)m-o-* '7th' and **dék̑m̥m-o-* '10th'. This idea goes back at least to Grimm (1831:634),[16] and the only more recent author I have found who has allowed even a flicker of doubt to cross his mind on the matter is Szemerényi (1960a:91) who, after referring with apparent approval to Benveniste's explanation (1948:161–2) of why the superlative should have adopted ordinal suffixes, adds 'If this is true, then the innovation is even more singular than it has been thought to date.'

13. And yet the proposed derivation runs into difficulties which in my opinion are insurmountable. This puts me in the delicate position of disagreeing with (almost) every scholar who has expressed an opinion on the subject in the last 135 years. I feel fairly confident that I am right; and yet it is embarrassing to have to say that if I am right, a large number of first-rate thinkers have not seen what seem to me rather obvious difficulties. To begin with the shape: formally the postulated ordinals can only be analyzed **sept(m̥)m-o-*, **dék̑m̥m-o-*, with a suffix *-o-*, not *-mo-*, added to the cardinals **septḿ* '7' and **dékm̥* '10'.[17] That a suffix *-mo-* should be detached from these is as unlikely as that E *seventh*, *tenth* should spawn a superlative *-enth* leading to forms like **upenth* 'highest', **downenth* 'lowest'. Kuryłowicz (1964:236–7) has seen this difficulty, but his solution for it is not very convincing.

14. More than that: Szemerényi has recently argued with vigor (1960a: esp. 70–86) that PIE did not have an ordinal **dek̑m̥mo-* at all, but only **dek̑m̥t-o-*, built on a form of the cardinal ending in a dental. While I am not quite totally persuaded that he is right, the bulk of the evidence does favor his view. Thus an archaic ordinal **dḱm̥t-ó-* is indicated by PIE **ḱm̥tóm* '100', which is most plausibly explained as 'the tenth (ten)' (Risch 1962); **dek̑m̥to-* would be a reapproximation to the cardinal **dékm̥(t)*. Also the contrast of Gk. *hébdomos* '7th' and *dékatos* '10th' points this way. If the less basic '7th' succeeded in keeping its old form without being remodeled to **héptatos* it is hard to see how the pivotal **dekm̥mo-* **dekmo-*

was so much less resistant to change.[18] Szemerényi is therefore probably right in considering Indo-Iranian *daśamá-*, Lat. *decimus*, and Celtic *decametos* to be independent innovations subsequent to the creation of a cardinal **dékm̥* in Indo-Iranian and Italo-Celtic.

15. But if this is true, then there was no PIE **dékm̥m-o-* available to be missegmented and form a superlative suffix *-(m̥)mo-*, and the whole burden of the innovation must rest on '7th'. Few, I think, will have the courage to claim that this one ordinal alone, belonging to one of the least salient of the digits, could have given rise to the whole array of Indo-European superlatives in *-(t)m̥mo-*.

16. The semantic side is just as difficult. For generations scholars have tried to explain what is the kernel of common meaning that permits ordinal suffixes to form superlatives and vice versa. The high point of this effort is Benveniste's impressive collection of material and acutely reasoned argument (1948:144–62). Benveniste's explanation is that both formations designate 'l'élément qui achève une totalité' (1948:162). This does not in fact seem quite accurate. It is true that ONE of the functions of ordinals in Indo-European languages is to mark the last of a series consisting of just so many items, as in Eddic *Gunnarr oc Hǫgni, enn þú, gramr, þriði* (*Grípisspá* 37.3–4). But I do not think that that was their basic function in PIE or that in PIE it was impossible to say things like, 'I shot seven arrows at the target; the first three and the last two missed, but the fourth and fifth hit.' As Brugmann says, (1906:163) the basic meaning of *the seventh day* is 'day number seven', the day characterized by having a relation of some kind to the number seven.

As for the superlatives, I do not think that the basic sense of a form like **up-m̥mo-* 'highest' is to designate the element that achieves a totality. In a contest to see who can stack the largest number of soda bottles, yes. But what if the game is to pick up a pile of objects one at a time without disturbing any of the remaining ones?

To be sure, one superlative does in fact regularly designate 'l'élément qui achève une totalité', the quasi-ordinal 'last', the antonym of 'first'. And since there is no denying that ordinals sometimes do designate the last of a series, there is indeed a semantic and psychological connection between the two formations. This semantic connection can well be a factor in the shaping of certain ordinals to look at least superficially like superlatives (cf. §§ 19–22); if the hundredth is also often the last, 'hundredth' can well be reshaped to resemble 'last'.[19] But the reverse does not follow; granted a language with ordinals but without a morphologic category 'superlative', there is no reason why the occasional use of 'hundredth' etc. to mark the last of a series should lead to its suffix being used as a superlative formant.

17. Aside from 'first' and 'last', the one semantic connection between the ordinal and the superlative is that both single out one particular object from a group (cf. Kuryłowicz 1964:230, 236). That is why languages with definite articles regularly use them with both ordinals and superlatives.

'The seventh', 'the best' are much commoner English collocations than are 'a best', 'a seventh'. Similarly, an American learning French is taught that in French you form the superlative by putting the definite article before the comparative: 'The French for "best" is le meilleur.' What English expresses somewhat redundantly by a combination of article and suffix replacement (*-est* for *-er*) French expresses more economically by the article alone.

It may be that this function of SINGLING OUT is responsible for the identity in shape between the ENTIRE SUFFIX of the ordinals **penkʷ-to-* '5th' and **s(w)eks-to-* '6th' and the SECOND COMPONENT of the superlative **-is-to-* that will be discussed below; but it can hardly account for the whole of the superlative *-(m̥)mo-*.

18. In the long list of ordinal formations from non-Indo-European languages assembled by Benveniste (1948:145–54), there is none that is specifically mentioned as also forming superlatives. Perhaps this is only because Benveniste did not think it necessary to present evidence on this point. But I suspect that an examination of the world's languages would reveal few or none that use the same morphologic process to form both superlatives and ordinals. Certainly none of the few non-Indo-European languages where I have information on this point do so. Thus in Finnish the superlative suffix is *-imp(a)-*, e.g. *vanha* 'old', *vanhin* 'oldest', essive *vanhimpana*, but the ordinal suffix is *-nt(e)-*, e.g. *neljä* 'four', *neljäs* 'fourth', essive *neljäntenä*. In Arabic the closest thing to a superlative is the elative, formed to the pattern *'af'alu*, e.g. *kabīrun* 'great', *'akbaru* 'greater', *'al'akbaru* 'the greatest', but the ordinals from '2nd' to '10th' follow the pattern *fā'il-*, like active participles: *xamsun* '5', *'alxāmisu* 'the fifth', lit. 'the fiver'.

19. Within Indo-European, always excepting the unique 'first', the Germanic set represented by NHG *der zwanzigste* etc. is the only case I have found where ordinals and superlatives have come to be formed alike without there being some satisfactory explanation that does not require us to suppose that the two formations were psychologically identified. Indeed, I suspect that the precise formal parallelism of sets like *dreissig*, *dreissigste* and *fleissig*, *fleissigste*, noted by Benveniste (1948:161), played an important part in leading Grimm and others to uncritical acceptance of apparent agreements elsewhere.

But it is significant that the agreement is not found, ordinarily, in the basic ordinals '2nd' to '10th', but rather in the higher ordinals only, from '20th' onward. Furthermore the evidence indicates that it is an innovation of Continental West Germanic. During the 'early' period—i.e. before 1100 A.D. or so—ordinals above '19th' are attested in this area only for Old High German, where '20th' to '100th' are consistently *zweinzugosto* etc., but *-osto* never penetrates into the series from *ander* '2nd' to *niuntazehanto* '19th'.[20] Outside Continental West Germanic evidence from Gothic is lacking for ordinals above *fimftataihunda* '15th', but OE *twentigoþa* etc. and OIc. *tottogonde* etc. attest formations bearing no resemblance to the OHG type.

By the time evidence is available for northern Continental West Germanic—thirteenth century or so—these dialects follow the High German pattern: MDu. *twintichste*, MLG *twintigeste*, OFris. *twintigoste* etc., and these have remained firm in Continental West Germanic since then. Sometimes *-st-* spreads into the lower numbers as well, as in MHG *vünfste*, MDu. *vijfste* '5th'. In view of the early and basic agreements of Saxon and Frisian with English against the dialects to the south, it is probable that at least in Middle Low German and Frisian the *-st-* forms are due to diffusion from the south, and that their center of origin was in the Frankish-Bavarian-Alemannic area. Without undertaking a full-scale investigation I cannot say whether the Middle English forms of the type *twentiest* (Brunner 1948:61) represent a second occurrence of the same innovation or are due to influence from the continent; but I think it altogether likely that the *tuttugasti* etc. of Modern Icelandic are due to influence from Northern Germany.

Although I cannot now give an adequate explanation of the OHG formation in *-osto*, I can at least point out the context in which an explanation is to be sought. To begin with, it is likely that in PIE and most of its descendants until fairly modern times, ordinals above '10th' were of but rare occurrence, and subject to a great deal of analogic reshaping and nonce formation. For '20th' and upward in Germanic this would have been particularly true after the PIE cardinals '20' to '120' were replaced by new formations of the type **twai tigiwiz* (acc. **twanz tegunz*) '20', Go. *sibuntehund* '70', etc. How were ordinals to be made to forms like these?

What the speakers of Proto-Germanic and Gothic did we do not know. North Germanic *tottogonde* beside *tottogo* '20' (the fossilized accusative **twanz tegunz*) clearly follows the pattern of *tíonde* '10th' (< **tehundan-*) beside *tío* '10' (< **tehun*), and cannot well have arisen before the merger of PGmc. **-un* and **-unz* in Primitive Norse. Similarly, OE *twentigoþa* to *twentig* imitates the relation of *tēoþa* '10th' to **tēon* '10' (preserved in Nh. *tēo*, *tēa*).[21] Since the pattern **tehun* : **tehun-þan-* = *twentig* : X would have resulted in **twentigþa*, it is probable that *twentigoþa* was created before the common West Germanic loss of the vowel that originally followed the *g* of *-tig*. If so, the Old English type has some right to be considered Proto-West Germanic.

It seems at least possible that the Old High German forms were created by Franks living among Romance-speaking people in what is now France. It is tempting to speculate that in their new environment these Germans had much more occasion than previously to use large numbers, especially when dealing with the Latin and/or Gallo-Romance speaking tenants, merchants, and bureaucracy. Now, as it happens, the Latin ordinals from '20th' onward, *vīcēsimus*, etc., had an ending very much like the superlative suffix *-issimus*: indeed, in the Vulgar Latin of Gaul, the two would have fallen together as **-ęsmos*. That there was some confusion of the two seems clear from the influence that the Old French ordinals had on the elatives in *-isme* borrowed from Latin (see below, § 22). In this context it seems

very possible that the German bumpkins would have gotten the idea that the clever and sophisticated Walha made the ordinal of '20' etc. by saying 'twentiest', and so formed *zweinzugōsto* etc., which they then passed on to their still more backward cousins east of the Rhine. It is not irrelevant that the Latin word glossed by OHG *zueinzicozstin* in the Benedictine Rule is spelled *uicissimo*.[22]

20. Other cases where Indo-European ordinals appear to have a superlative suffix prove illusory. At first blush the type of Lat. *vīcē(n)simus* '20th', *trīcē(n)simus* '30th', Skt. *viṃśatitamá-* '20th', *triṃśattamá-* '30th', Av. *vīsąstəma-* '20th', *θrisastəma-* '30th' seems to be formed with the superlative suffix *-tṃmo-*. But these have recently been explained much better by Szemerényi (1960a:90–1), following Brugmann (1903:6): the PIE formation was in *-to-* (itself spread from **penkʷ-to-* '5th', **s(w)éks-to-* '6th', and perhaps **deḱṃto-* beside **deḱṃ*), as preserved in Gk. *eikostós* etc. In Italic and Indo-Iranian this suffix was enlarged by the final of *decimus*, *daśama-*. That is, the *-mo-* of '7th' and '10th' has indeed spread by analogy, but only to other ordinals.[23] In Indo-Iranian this was possible at least as soon as final **-ṃ* had become *-a*, entailing automatically a new segmentation *sapta-má-*, *daśa-má-*, which led by flawless proportional analogy to *pañcamá-* '5th', *aṣṭa-má-* '8th', *nava-má-* '9th', and by reinforcement of an already existing suffix to *vimśati-ta-má-* etc. No doubt the superlatives in *-tama-* played a supporting rôle; but only a supporting one.

In Latin **-ṃ* and **-ṃm-* both gave *Vm*[24] so that *septim-u-s* and *decim-u-s* could not easily be resegmented—hence the failure of *-mo-* to spread within the digits before the decay of final syllables led to OFr. *set* '7', *setme* '7th', whence *uitme* '8th' and *nuefme* '9th' beside *uit*, *nuef*—until the creation of forms like *decuria*, *deciēns*, *dēnī*, whose origin and date are not clear. It seems therefore that the extension of *-mo-* to ordinals above '19th' is mainly due to a sort of homoioteleuton: in langue, if not in parole, '20th' followed an unbroken string of ten ordinals in *-mus*, from *decimus* to *nōnus decimus* (insofar as *ūndēvīcēsimus* etc. weren't used), and it was natural to go on in the same fashion. Cf. the somewhat similar carryover of Lat. *-ēsimus* from *nōnāgēsimus* to *cent-ēsimus* and of Gk. *-ostós* from *enenēkostós* to *hekat-ostós*.

21. Homeric *trítatos*, *hebdomátēi*, *ogdoátōi* beside normal *trítos*, *hébdomos*, *ógdoos* are not formed after the superlatives—*prṓtistos* shows what the result would have been in that case—but rather after *tétratos*, *é(i)natos*, *dékatos*, and owe their existence largely to the impossibility of fitting several case forms of *trítos* etc. into hexameter. *Pémptos* and *héktos* presented no such metrical problems, and so there are no Homeric **pémptatos*, **héktatos*.[25]

22. Likewise the chief reasons that in Old French there was interaction between the ordinals and the 'superlatives' formed with *-i(s)me* borrowed from Latin are the close formal similarity between their suffixes to begin with and the fact that both formed adjectives derived from other adjec-

tives. But the more precise semantic connections alleged by Gilliéron (1920:106) and quoted by Benveniste (1948:161) seem to me unreal.

In fact, it appears that the existence of adjectives like *sainti(s)me* 'most holy' contributes nothing whatever to explaining the triplicity of forms in the Old French ordinal suffix *-i(s)me*, *-e(s)me*, *-ie(s)me*, and is of rather dubious value in explaining why this suffix spread from '11th' and higher numbers to '10th' and the digits. It is rather the existence of rival forms *onzi(s)me*, *onze(s)me*, *onzie(s)me* that explains the occasional appearance beside *sainti(s)me* of *sainte(s)me* and *saintie(s)me*. A man laughed at for saying *onzi(s)me* instead of *onzie(s)me* might well apply the lesson he had just learned to other words ending in *-i(s)me*, especially to words with a formal and serious flavor, as the learned formations in *-i(s)me* were.

I cannot presume to say anything definitive about the true history of the French ordinals here. But it appears that Clédat, in his short but trenchant comment at the end of Gilliéron's article, is essentially right: *-i(s)me* arose from *-ecimus* in *onzi(s)me* etc., and *-e(s)me* from *-ēsimus* in *centēsimus* etc. The two encroached on each other's territory, resulting in *onze(s)me*, *centi(s)me*, etc., and apparently *-ie(s)me* arose from a contamination of the two. I have not found a source that would permit me as a non-specialist to find out anything about the history of the replacement of the set *altre . . . di(s)me* by *deuxième . . . dixième*; but it appears that the easiest hypothesis, that it began among the most irregular pairs like *deux* : *altre, trois* : *tiers* will not work, and that instead it was the less frequently used, albeit more regular *setme*, *uitme*, *nuefme* that were the first to go. However that may be, I do not think that the 'superlatives' need have been in the language for *-ie(s)me* to spread.

One final remark: is it possible that **viesmes* '20th' from Lat. *vīgēsimus* existed at a time when it could have given rise to the puzzling variant *-iesme*?

23. Watkins (1966:37) remarks that the relation of the Celtic ordinals to the superlative formation deserves fuller exploration. On looking into this, I find only that the Celtic ordinal suffix *-(a)meto-* and the Celtic superlatives suffix *-isamo-* both contain five phonemes arranged in the pattern VCVCV, that both include the sequence *-am-*, and both are *o*-stems. The early history of the ordinal suffix is adequately sketched by Thurneysen (1946:250), but it may do no harm to repeat it here in more detail. To begin with, **septmos* (**sebdmos*) '7th' and **dekṃtos* '10th' were refashioned in both Italic and Celtic to **septṃm-o-s*, **dekṃm-o-s* after the cardinals **septṃ*, **dekṃ*.[26] As soon as final **-m* had become *-n* in Celtic, the pattern **dekan* : **dekamos*, **sektan* : **sektamos* allowed **nawanos* '9th' to become **nawamos* (just as OFr. *nuefme* results from the loss of final syllables in Gallo-Romance). **oktū* '8' was now flanked by two ordinals formed by a rule 'replace final consonant by *-mos*'; it too fell into line, but, lacking a final consonant, simply added *-mos*, and with **oktūmos* '8th', *-mo-* was launched as a true ordinal suffix in Celtic.

Meanwhile innovation was starting at another point in the ordinal

system. *k^wink^{w}e '5' replaced its ordinal *k^winktos, which was probably pronounced approximately [kṷĩ:xtos], by the more regular and transparent *k^wink^{w}e-to-s, whence Gaul. *pinpetos*, OIr. *cóiced*, MW *pymhet*. This new ordinal was now cut k^wink^w-eto-s, setting free a second new suffix, *-eto-*. The reason for this new cut has not been made clear as far as I know. When Arm. *hinge-rord* '5th' was reanalyzed as *hing-erord* giving rise to *vec'-erord* '6th' and the like, the reason was clearly the loss of the *-e that originally stood at the end of *hing* '5'. Now a general loss of PIE *-e in Proto-Celtic is out of the question, but there are one or two bits of evidence that it happened at least occasionally, more or less as Latin *dīc dūc fac hoc(c) seu* coexisted with *finge age ecce sīve*. Welsh *fyN* 'my' instead of **fyfL* from **meme* might be discounted as proclitic and *-k 'and' as enclitic, but the curious short imperatives listed by Thurneysen (1946:375)—*atræ* 'arise', *aicc* 'invoke as surety' etc.—are much more naturally and simply explained as coming from apocopated **reg(e)*, *g^wed(i) etc. than as 'injunctive' *s*-aorist forms that have mysteriously been incorporated into the Irish imperative system.

Whether *k^wink^{w}e had beside it an apocopated *k^wink$^{(w)}$ or not, the fact remains that *-eto-* was cut loose, and proceeded immediately to the next ordinal in the series, replacing **swek(s)-to-s* '6th' by **sweks-eto-s*, the ancestor of OIr. *sessed*, MW *chwechet*.[27] Applied to **sektan* '7' it naturally resulted in **sektam-eto-s* replacing **sektam-o-s*, whence Gaul. *sextametos*, OIr. *sechtmad*, MW *seithfet*. In the same way arose the ancestors of Gaul. *oxtumeto*[*namet*[*decametos*, OIr. *ochtmad nómad dechmad*, MW *wythfet nawfet decfet*. By the same sort of process that we have already observed in Lat. *vīcēsimus centēsimus* and Skt. *viṃśatitamá-* the ending spread (only after the loss of Proto-Celtic final syllables around the middle of the first millennium A.D.?) to higher numerals, OIr. *fichetmad*, *cétmad*, MW *ugeinfet*, *canfet*, etc. In Old Irish there was also a backlash to '4th', where the **cethr(a)e* that would have corresponded to MW *pedweryd* was replaced by *cethramad*. All of this history has nothing to do with the development of the superlative suffixes in Celtic.

24. But the ordinal 'first' not merely looks like a superlative in many Indo-European languages, it IS a superlative, often with a transparent etymology 'foremost, earliest'.[28] Umbrian *promo-* has already been mentioned in § 8. Latin *prīmus* (**pri-is-m̥mo-*) is the superlative to *prior* 'earlier, former'. Go. *fruma*, *frumists*, OIc. *fyrstr*, OE *forma*, *formesta*, *fyresta*, OHG *furisto* are all superlative formations to the same *pr-* adverbial base, while OE *ǣresta*, OHG *ēristo* belong with Go. *air* 'early'. Skt. *pra-thamá-*, OP *fra-tama-*, Av. *fra-təma-* are correlated with Skt. *pra-tará-m* 'further (more to the front)' and Av. *fra-tara-* 'former'. Gk. *prôtos*, *prâtos* must be from **pró-ato-s* beside *pró-tero-s* 'former',[29] despite the difficulty about the contraction of **oa* to *ā* in West Greek and Boeotian: I was wrong to deny this (Cowgill 1965:149).[30] The **pr̥̄-mo-* underlying OPru. *pirmois*, Li. *pìrmas*, Latv. *pìrmais* can be taken either as a 'superlative' formation or as an altered form of the **pr̥̄-wo-* of OCS *prŭvŭ* 'first', Skt. *pū́rva-* 'preceding,

former, ancient'. OW *cisemic* 'primus' and MW *cysefin* 'first, original' are based on a **kinssamo-* that seems best analyzed as another *-tm̥mo-* derivative (like *gwarthaf*, *eithaf*), to the **kint* that underlies MW *cyn(t)* 'eher, schneller'; MW *cyntaf*, Co. *kynsa*, MBr. *quentaf* are replacements of this with the regular British superlative suffix *-(h)av* added to the shapes that MW *cynt*, Co. *kyns*, Br. *quent* had acquired.[31]

The reason for all this is obvious: unlike the ordinals from '2nd' onward, which cannot be at the beginning of a series and need not be at its end, the first item is always and inescapably at one end of the series to which it belongs. It contrasts not with another member of the series located with reference to its absolute position—'third', 'three millionth' or what have you—but with the member located at the opposite end of the series, the last.

25. It is here, if anywhere, that a genetic connection between the ordinals and the superlatives in *-(m̥)mo-* is to be sought. The PIE suffix **-tyo-* is added to adverbs in Sanskrit to form adjectives with the meaning 'in der durch das Adv. bezeichneten Ortslage befindlich' (Wackernagel and Debrunner 1954:697). Added to adverbs in Hittite it forms adjectives like *hantezziš* 'first', *appezziš* 'last' whose natural English translations are superlatives even though that category did not exist in Hittite. It seems reasonable that in similar fashion an originally colorless suffix *-mo-*, seen for instance in Skt. *bhī-má-* 'frightful' beside *bhī́-* 'fear' or Gk. *ther-mó-s* 'warm' beside *thér-os* 'hot season', when added to adverbs like **pro* 'in front, forward' **up(o)* 'upward' would form adjectives 'the one in front', 'the one at the top' whose meaning could very easily be specialized as 'foremost, first' and 'topmost, highest'.

26. To return to the stated theme of this paper. While Italic and Celtic reveal nothing exciting in their treatment of the PIE *-(t)m̥mo-* superlatives, their treatment of the superlatives associated with the *-yos-* comparatives is quite distinctive. It will be recalled that *-yos-* is the regular comparative suffix in Italic and Celtic (and Germanic), and we might therefore expect that the regular superlative of Italic and Celtic will be the one correlated with this *-yos-* formation.

27. Germanic, Greek, and Indo-Iranian agree in forming the superlative to *-yos-* not by substituting an altogether different suffix for it, as was the case with *-(t)ero-/-(t)m̥mo-*, but by adding a secondary suffix *-to-* to the zero grade of *-yos-*, resulting in a composite *-is-to-*: a favorite example is OE *swētest*, Gk. *hḗdistos*, Skt. *svā́diṣṭha-* (with secondary *ṭh*),[32] PIE **swā́d-is-to-* 'sweetest'. This *-to-* may be identical with the *-to-* of **pénkʷ-to-* '5th' and **s(w)éḱs-to-* '6th', but it is probably not borrowed from it. Rather *pénkʷ-to-* 'LE cinq(uième)' and *swā́dis-to-* 'LE plus doux' are derived from *pénkʷe* 'cinq' and *swā́dyos-* 'plus doux' by means of the same individualizing suffix.

28. In Italic and Celtic *-m̥mo-* rather than *-to-* is added to the comparative, resulting in *-is-m̥mo-*, which underlies all the Italic and Celtic superlatives that have not already been discussed. The principal aim of this paper is to show that this is true, and that the differences mentioned by Watkins (1966:36–7) are all the result of secondary changes later than the creation of a common Italo-Celtic superlative in *-ism̥mo-*.

29. Whether an isolated relic of *-isto-* is found in Lat. *juxtā*, *juxtim* 'next to' is at best extremely uncertain, and in any case of no importance for tracing the history of *-isṃmo-* once the latter had gotten started. One may think as well of a compound with a form from the root **stā-* 'stand'.

30. In Celtic it is well known that the regular superlative endings OIr. *-em*, *-am*, W. *-(h)af*, Co. *-a*, Br. *-(h)af(f)*—e.g. OIr. *dilem* 'dearest', OW *hinham* 'patricius', MW *teckaf* 'fairest'—come directly from **-isṃmo-* through Proto-Celtic **-isamo-*. Of the possible Continental Celtic examples mentioned by Pedersen (1913:122) Lewis and Pedersen (1937:185), and Schmoll (1959:44–5), the place names *Ouxisamē* 'highest?' (cf. the tribe *Osismi*), *Trigisamum*, *Segisama* 'most victorious?', and *Bletisama*, *L-e-ta-i-ś-a-m-a* (modern *Ledesma*) 'broadest?' and the divine names *Belisama* and *Rigisamus* 'most royal?' are formally unimpeachable as superlatives, whatever the real meaning of each may be. To them can be added the modern place name *Monesma*, on the assumption that its syncope is late, like that of *Ledesma*. The personal name *Blesam(i)us* can belong if its *e* represents a fusion of root vowel and suffixal *i*, and the place name *Rixama* may have a relatively early syncope of *-i-*. But the personal name *Cintusmus*, lacking both *i* and *a*, can hardly be a superlative, despite the attractive conjecture that it means 'firstborn', 'foremost'. On the Spanish place name *Uxama*, modern *Osma*, see § 46 below.

31. In Latin numerous well-known examples are preserved. Thus *prīmus* 'first' beside *prior* 'former' from **pri-yōs* continues **pri-is-ṃmo-* = **prīsṃmo-*. By syncope this became *prīsmo-* (plausibly attested in the Paelignian name *Prismu* 'Prima'), and by regular Latin loss of **s* before *m* (together with compensatory lengthening of the preceding vowel, which in this case had no visible effect as the vowel was already long) *prīmus*.

Likewise *plūrimus* 'most' beside *plūs* 'more' must be in the end a continuation of **pleE-is-ṃmo-*, despite the difficulties with the vocalism (on which see Szemerényi 1966a:n50).

**-isṃmo-* is also commonly recognized in the Latin superlatives in *-errimus* and *-illimus*, which are regularly and rightly derived after the schemes **pigr-isṃmo-s* > **pigrsemos* > *pigerrimus* and **fakl-isṃmo-s* > **faklsemos* > *facillimus*, with syncope of the second syllable (as in *prīmus*) and assimilation of **s* to a preceding liquid.[33]

32. In some other superlatives Latin phonology would permit a suffix **-sṃmo-* rather than **-isṃmo-*, but the principle that a comparative in *-yos-* requires as partner a superlative in *-isṃmo-* leads us to a correct interpretation. Thus *maximus* 'biggest' beside *magis* 'more', *maior* 'bigger' can only be from **mag-isṃmo-s*. Likewise *proximus* 'closest' beside *propior* 'closer' must be **prok^{w}-isṃmo-* or **prop-isṃmo-*, despite the unexplained interchange between labial and (labio)velar. And *pessimus* 'worst' beside *peior* 'worse', whatever may be its ultimate etymology, is syncopated from an immediate preform **ped-isemos*, **pess-isemos*, or the like.

The same applies to the *oxime* 'ocissime' cited by Paulus-Festus. *Medioximus* 'midmost' instead of **mediocerrimus* beside *mediocris* either is based on a form of the adjective without the *-ri-* suffix or reflects complete loss of

the *r* in syncopated **mediokrsemos*, but in no case requires a suffix **-sm̥mo-*. Similarly *clārimum* and *pūrimē*, as stated by Sommer (1900:230).

33. A group containing this suffix that appears not to have been recognized yet is the triad *extrēmus postrēmus suprēmus*. Sommer (1900:253) suggested they were made by adding *-smo-* (!) to **extero-* etc. at a time when these were already syncopated in some cases and their stem vowel weakened to *e*; **extre-smo-* would give *extrēmus* regularly. The weakness of this construction hardly needs to be pointed out. Brugmann (1903:14) suggested rather that they began with formation of *suprēmus* on the pattern of **dē-mo-s* 'lowest, last' an adjective underlying the adverb *dēmum* 'finally', and this explanation was adopted by Sommer (1914:460) and Leumann (1928:220). But the existence of an adjective **dēmus* in the required meaning at the required time is altogether uncertain, and the pattern *dē* : *dēmus* should have led to *super* : *supermus*.

However, beside the contrastives *extero- postero- supero-*, which in Latin occur mostly in the specialized senses 'outsider, foreigner', 'descendant', 'celestial', there were true comparatives *exterior posterior superior*. If we recall that **i* became *e* before voiced **s* in the prehistory of Latin—cf. *serō* < **sisō*, *fēcerit* beside *fēcisset*—we can easily derive *suprēmus* etc. from **super-isemo-s* etc. through **supresmos* (vowel lowering and syncope of alternate syllables beginning with the posttonic),[34] *suprēmus*. I.e. they are the regular and expected superlatives beside *superior* etc.

34. Other cases where Latin appears to have a simple *-(m̥)mo-* outside the *-tero-/-tm̥mo-* set are more difficult, but I believe are to be interpreted in the light of what has preceded.

Brūma 'winter solstice' is no doubt originally a superlative to *brevis* 'short', either more archaic in formation than standard *brevissima*, or from a different dialect. But a preform **breu̯imā* (Sommer 1914:80) or **brehumā*, **brex̯imā* (Leumann 1928:297), with the wrong superlative formant, is out of the question.

The basic difficulty is that we do not understand the shape of *brevis* itself. A PIE **mr̥ǵh-ú-* 'short' is adequately attested by Gk. *brakhús*, OHG *murga* (f), Av. *mərəzu-*, Sogd. *mwrzk-*, Saka *mulysga-* 'short' (these last two from **mr̥zu-ka-*), OE *myrge* 'kurzweilig, pleasant', Go. *gamaurgjan* 'shorten', Skt. *múhu(ḥ)* 'suddenly' (with Middle Indic treatment of *r̥*). With the regular Latin addition of *-i-* to *u*-adjectives, this should have become pre-Lat. **morxwis*, whence Lat. **morguis* (cf. *ninguit* < **sninxwet*) or possibly **morvis*. But *brevis* seems to require a preform **mrexwis*, and if we are not to assume ad hoc an otherwise totally unparalleled development of **r̥* to *re* in some Italic dialect, we must explain the *re* as a replacement for *r̥* or *or* under the influence of some other form.

In that case the most likely culprit is the comparative (and superlative; on the original full grade of the *-isto-* superlatives I agree completely with Meillet 1900) where the root of **mr̥ǵh-ú-* should have appeared in full grade; cf. e.g. Skt. *ur-ú-* 'broad', *vár-īyas-* 'broader', *vár-iṣṭha-* 'broadest'. As full grade to **mr̥ǵh-*, **merǵh-* and **mreǵh-* are equally possible by the general

rules of PIE ablaut. Unfortunately we have no evidence outside of Latin as to which of the two actually existed. The Saka comparative *mulśa* (Konow 1949:119) I take to be from **mr̥z-yah-*, with the vocalism of the positive carried through, much as in NE *merrier*. It is by no means sure that Gk. *brakhíōn* 'upper arm' and *brássōn*, a Homeric hapax of uncertain meaning,[35] are really comparatives to *brakhús* (Frisk 1960:264); if they are, they too can have adopted the vocalism of the positive, as is no doubt also the case with Hesychius' *bróssonos*: *brakhutérou*, presumably from a dialect where *o* rather than *a* developed beside syllabic **r̥*.

We are thus left with Lat. *brevis brevior* as the only evidence for an original comparative **mreǵh-yos-*; if we accept it, we have also to admit that contrary to the general rule the positive was remodeled after the comparative, rather than vice versa. The situation is complicated still more by the existence in Latin of two other *-vis* adjectives with unexplained vocalism, *levis* and *gravis*. Especially *levis*, belonging to the same semantic area as *brevis* and riming with it, may be relevant to the path by which **mr̥ǵhú-* became *brevis*.

So if we are to hazard a preform for *brūma*, the best guess but a very weakly supported one is **mreǵh-isṃmā*. Assuming mutual approximation of superlative and positive, we can get pre-Lat. **mrexwis*, **mrexwisemā*. Now the latter will become *brūma* if we can get rid of the *x* in time to take advantage of the sound law **ew* > *ow* (as in *novus* < **newos*), which would then set things up for the change *owi* > *ū* (as in *prūdēns* < **pro-wident-s*), after which the rest is easy: **mrexwisemā* > **brewisemā* > **browisemā* > **brousemā* (**broisemā*?) > **brousmā* (**broismā*) > *brūma*.

The difficulty is that *ew* from **exw* is not supposed to turn into *ow* in Latin, but the evidence for this is precisely the two problematic items *brevis* and *levis* (Sommer 1914:59, Leumann 1928:136). It seems legitimate to suggest therefore that *brūma* shows the regular outcome of **exwi*, at least in a tetrasyllable, and that *brevis*, *levis*, whose vocalism cannot be original anyhow, either attest a different outcome in disyllables or had not yet acquired their *e* at the time of the change *ew* > *ow*. But if that is so, then the ancestor of *brūma* need not be **mrexwisemā* after all; it might be **mroxwisema*, i.e. **morx-* metathesized for some unclear reason, and *brevis* would then be a late replacement for **brovis* (> **bravis*?). Whatever the truth of the matter, *brūma* cannot be taken as evidence for a simple *-mo-* superlative among the ordinary adjectives of Latin.

35. Vulgar Latin *ipsimus*, *ipsima*, used by Trimalchio in the meaning 'master', 'mistress', is clearly a component of OFr. *me(d)esme*, *me(d)isme* 'self, same'. (Meyer-Lübke 1935:455 and Rohlfs 1949:249 say that Sp. *mismo*, Ptg. *mesmo*, It. *medesimo* and the like are borrowed from Gallo-Romance. While I lack the knowledge necessary to judge this view, it looks very plausible).

It seems to be universally accepted that this *ipsimus* is a superlative of *ipse* '(him)self'. But how can this be? Formally, there were no models available in Classical Latin for a set *ipse* : *ipsi-mus*. The superlative to *ipse*,

when one was made for special effect, as in Plautus' *Trinummus* 988, was *ipsissimus*, formed with the regular standard Latin superlative suffix *-issimus*.

Nor is there anything superlative about the meaning of *ipsimus*. Objectively it differs not at all from *ipse*—Trimalchio's sense is only a bit of slang, common to many languages, and equally appropriate to *ipse*—and it is not easy to see how 'his very own self' would have ever been used except with the intent of saying something witty or original, and so become a rival and eventual supplanter of the ordinary *ipse*. NHG *selbst* is no evidence to the contrary, as a glance at Kluge-Götze (1951:717) will show.

Ipsimus must be a colloquial replacement of *ipse*; although the way in which it was formed is not now clear, there is no good reason to consider it a superlative in shape.[36]

36. *Minimus minor* beside *parvus* are an isolated and irregular set. It is generally believed that *minor*, not **minior*, cannot be a comparative of the usual type; the commonly accepted view that its inflection is based on the neuter *minus* from **minwos* (Sommer 1900:63) is inferior to Szemerényi's recent derivation from **minu-yōs* (1966a:n47). It is ordinarily not recognized that *minimus* cannot well be *minu-mo-s*, with *-mo-* instead of *-isṃmo-*. My present best explanation of *minimus* is that it is formed by a kind of analogy to *minor*. As *minor* appeared to be derived from a base *min-* (appearing also in the verb *minuō* 'lessen') with a suffix *-or* that differed from the ordinary comparative *-ior* by lacking its first part, so a superlative might be formed to the same base *min-* with an equally mutilated suffix *-imus* in place of *-is(s)imus*.[37]

37. As for the commonest Latin superlative suffix, *-issimus*, it is wrong to put any weight on the obscurity of this form as an argument against common innovation in Italic and Celtic. *-issimus* is specifically Latin (where it is attested at least from the time of Naevius, in the second half of the third century B.C.), not recurring even in Osco-Umbrian. It cannot prove that a difference existed between Italic and Celtic already in the second millennium B.C., especially not when such abundant traces of the common Italo-Celtic *-isṃmo-* are preserved in Latin.

On the source of this suffix I have nothing new to add here. But I will point out that the *-ist(o)-sṃmo-* favored by Sommer (1914:460) is impossible; we may be quite sure that there were no *-isto-* superlatives in Latin when **-isemo-* was transformed into *-issimo-*. Brugmann's *-is-sṃmo-* (1903:13) is impossible too, not only for the reasons given by Sommer (loc. cit. and 1914a:127f.), but because, as I shall show below, there never was an independent suffix *-sṃmo-* in Italic or Celtic. Two things that should be kept in mind in working toward an adequate solution are the likelihood of expressive gemination in a form charged with expressive meaning like the superlative—compare for instance the Old Irish double superlatives in *-imem -emem -amam* of the Milan Codex, assembled by Sommer (1900:223–4)—and the need for a uniform suffix in the face of Latin phonology, which would have led to sets something like *novus* : **nūmus*, *altus* :

**alsimus, cal(i)dus* : **cal(i)dēmus, prāvus* : **praurimus*. While such developments could be tolerated in a few frequent words like *maximus* and *plūrimus*, and limited formal sets like the types in *-rēmus -errimus -illimus*, the language needed for the mass of its adjectives a single unvarying suffix that could be added to any and all alike.

38. The supposed Italo-Celtic superlative suffix *-sṃmo-* mentioned in the preceding paragraph was first posited by Brugmann (1889:169). As Sommer rightly says (1900:232), the hypothesis of such a suffix 'muss ... für jeden, der allein vom Lateinischen oder Italischen ausgeht, den Eindruck einer höchst künstlichen und überflüssigen Konstruktion machen.' Instead of the firmly established and well integrated pairs *-(t)ero-/-(t)ṃmo-* and *-yos-/-is-ṃmo-* that we have met so far, we are asked to believe that somehow the suffix *-ṃmo-* found itself being added not to local adverbs, but to zero grades of *s*-stem nouns (or roots with *-s-* extensions?), and that then somehow the preceding *-s-* got detached from the stem to which it properly belonged and stuck on to the *-ṃmo-*, resulting in a conglomerate *-sṃmo-* which might have been expected to appear as a competitor of *-tṃmo-* in all the IE languages that have superlatives, but instead appears only in Italic and Celtic, and there largely in adjectives that ought to have the *isṃmo-* superlative. Truly a wonderful and marvelous set of events, and one not to be credited except in the face of overwhelming evidence.

39. The evidence that led Brugmann to posit his *-sṃmo-* and serious and careful scholars like Sommer to accept it is essentially Celtic. Besides the regular superlatives that can be traced back to **-isṃmo-* with no difficulty, Celtic offers the following:

Gaulish	Old Irish	MWelsh	Cornish	Breton	'Proto-Celtic'	
	tressam	trechaf			*treksamos	'strongest'
neδδamo-	nessam	nessaf	nessa	nessaff OB nesham	*nessamos	'nearest'
	messam				*messamos	'worst'
Celtib. Uxama	(úaislem)	uchaf	(uhella)	(uhelhaff)	*(o)uksamos	'highest'
	(íslem)	isaf, hyshaf	(ysella)	(iselhaff)	*īssamos	'lowest'
		hawsaf			*sāssamos	'easiest'
	máam, móam	(mwy(h)af)	(moygha)	(muyhaff) (OB meham)	*maisamos	'greatest'
	óam	(ieu(h)af)			*yowisamos	'youngest'
	síam	(hwyaf)		(OB huiam)	*sēsamos	'longest'

The Old Irish is quoted from Thurneysen (1946:235); the Welsh from Evans (1964:40–1); Cornish from Lewis (1946:18–9); Breton from Lewis (1935:14) and Fleuriot (1964:251). I have omitted items that are clearly not superlative in form, like OIr. *dech*, MW *goreu* 'best', and what I take to be insignificant spelling variations. The last three forms are in the list not because anyone seriously doubts that they are formed with the *-isṃmo-* suffix

—PC **sēsamos* is regular from **seE-is*—but because their precise shape in Irish will call for a bit of comment.

40. Some of these have cognates in the Italic languages. Thus the Oscan and Umbrian for 'nearest' is *nessimo-*, well attested in both languages, and obviously related to Celtic *nessamo-*. To OIr. *messam* may correspond O **messimass** in a damaged inscription (Vetter 1954:no. 86): if the Oscan word means 'midmost', it is thinkable that the Irish word once had the same meaning, but underwent a shift from 'average' to 'mediocre' to 'worst'. However, the step from 'mediocre, inferior' to 'worst' is a long one; and in view of the clear formula of Vetter no. 81, **nessimas .staíet/veruís** 'they stand next to the gates', I cannot help wondering whether **me/ssimasstaief/** in no. 86 is a mistake for **ne/ssimas .staiet** (the final consonant of **staief** may be assimilated to the following **fud**). A decision on this point is of no importance for this paper, since the explanation I have for *nessimo-*, **nessamo-* will apply equally well to **messimo-**, **messamo-* if such an equation exists.

The town *Auximum* in Picenum (modern Osimo) may mean 'highest' (the town is on an isolated hill); the same may be true of Celtiberian *Uxama*, modern Osma, and both can then be compared to W *uchaf*.

O **imaden** (Vetter no. 8) has been taken to mean 'from the ground up', related to Lat. *īmus* 'lowest', and both derived from an **īdh-s-(m̥)mo-* supposedly underlying W *isaf* as well (Sommer 1900:208). But in fact, as Sommer (1914:457, 1914a:126) points out, it is not at all clear that O **imaden** means 'from the ground up'; and even if it does, it and Lat. *īmus* (a mostly poetic word, found in Cicero only once) may reflect only a remodeling of the ordinary *īnfimus* after its antonym *summus*. For Oscan this is not easy, since Umbr. *hondomu* and O **húntr-** (§ 10) indicate that the Oscan correspondent of *īnfimus* should have been **hontomo-*. But for Latin Sommer's explanation seems very likely; one can even set up a proportion *su-perior* : *su-mmus* = *īn-ferior* : X, X = **īn-mmus*, a virtual form that would automatically be realized as *īmus*. O **imaden** is then best left out of account.

41. For the other three—*nessimo- messimass Auximum*—it would be (putting aside for a moment the question of how **-m̥m-* came to yield *-im-*) perfectly possible to set up preforms **nedh-ism̥mo- *medh-ism̥mo- *auks-ism̥mo-* with the regular suffix *-ism̥mo-* and syncope of *-i-* (Sommer 1900: 232). The whole burden of proving an Italo-Celtic *-sm̥mo-* therefore rests on the first six Celtic forms listed in § 39.

42. There is a point about these which scholars have regularly noticed but failed to appreciate. In all six, the *-s-* of the superlative recurs in the comparative: OIr. *tressa nessa (nesso) messa*, MW *trech nes* (this also Cornish and Breton) *uch is haws*: and in most that have a positive, the *-s-* occurs there too: OIr. *trén*, MW *tren* 'strong' < **treks-no-* (**trekno-* or **tregno-* would have given MW **trein*); OIr. *úasal* MW *uchel* 'high', Gaul. *Uxellodunum*; OIr. *ísel* MW *isel* 'low'—cf. also the OIr. adverbs *úas* 'above' *ís* 'below', which do not function as comparatives. Only to MW *haws hawsaf* does the positive *hawd* apparently lack an **-s-*.

43. The obvious conclusion, one might think, is that the *-s-* of **treksamos* etc. is not part of the superlative suffix at all, but part of the base to which that suffix is added, and that we are really dealing only with some more examples of the *-ŋmo-* of Lat. *summus* and *īnfimus*. But as far as I know, the only scholar who has made this analysis is Kuryłowicz (1964:238n8). The orthodox view rather inconsistently admits that these comparatives and positives contain a root extension or suffix *-s-*, and yet uses **treksamo-* etc. as justification for deriving Lat. *maximus* etc. from **mag-sŋmo-* despite the lack of any *s*-extension in *magnus* and *maior*. Pedersen (1913:123) suggests that *-s-* spread from superlative to comparative in Celtic.[38]

44. While Kuryłowicz's view is far superior to the orthodox position, there is still the difficulty that adjectives meaning 'strong' and 'easy' should not have superlatives formed with the *-ŋmo-* suffix. I suggest rather that at least **treksamo-* and *hawsaf*, and probably the whole set, have suffered haplology of *-sis-* to *-s-*; **treksamo-* is for **treks-isamo-*, *nessamo-* for **ness-isamo-*, *ouksamo-* for *ouks-isamo-* (attested, it will be recalled, in the *Ouxisamē* mentioned in § 30), and so on. What we have here is perfectly regular superlatives, formed with the regular Italo-Celtic suffix *-isŋmo-*, which have merely become altered by a phonetic accident.

45. For the sound change compare the well-known Latin type of *dīxtī* beside *dīxistī*, and Szemerényi's recent and brilliant explanation of the Vedic type *jeṣi* by haplology of **jaisasi* to **jaisi* (Szemerényi 1966).

46. For **ouks-isamo-* this analysis presents no difficulties whatever, other than those connected with the Italian place name *Auximum*, whose vowel does not agree with Gaulish *Ouxisamē* and whose *x* is incompatible with Gk. *hupsēlós*; *Uxama*, *Osma* evidently has zero grade from the positive **upselo-*, while OIr. *úasal*, W *uchel* show the inverse analogy. For **īss-isamo-* the only problem is to find a plausible etymology, a problem that exists equally well if the superlative is taken as **īssamo-* (the **pēd-su* of Pedersen 1909:50 and Lewis and Pedersen 1937:21 is quite unlikely). Of course, **īssisamo-* can hardly be the source of Lat. *īmus* and Osc. **imad-** (§ 40), since it should give Lat. **īsimus* and Osc. ***isimo-** (cf. § 67).

47. For an etymology of Welsh *hawdd* 'easy' *haws* 'easier' *hawsaf* 'easiest' we need to find a base that will explain the alternation of /ð/ in the positive with *-s-* in the superlative such that the root vowel will be *aw* not only in monosyllables but also in disyllables (cf. also the equative MW *hawdet*, *hawset*, and contrast the normal development of Celtic **ā* in MW *tlawt tlottaf*). The latter problem I have not been able to solve. Morris Jones' derivation from PIE **swād-* 'sweet' (1913:247) is unsatisfactory both as to sense and as to final consonant, and his ad hoc sound law that British **swā-* in antepenult gives W *haw-* rather than the expected **chwo-* has nothing to recommend it.

But the alternation of /ð/ in positive with *-s-* in superlative can be explained by positing a base not in **d* but in **s*, with a *-yo-* suffix in the positive. The development of **-sy-* to W /ð/ is well established; see Pedersen (1909:69), Lewis and Pedersen (1937:15). This interpretation of *hawdd* throws some light on its Old Irish equivalent *asse*, with neutral /s/. If this

were **ad-sta-yo-*, the etymology favored by Vendryes (1959:96), the development to *asse* would be inexplicable. Even supposing that PIE **y* remained between *a* and a following vowel in Proto-Celtic, which is more than doubtful, it is quite certain that it was lost by primitive Irish times. But Prim. Ir. **assaas* could only have given OIr. **assa*, not *asse*. If instead we start from a form ending in **-ăsyos* or **-osyos*, the development through Prim. Ir. **-ăsias*, **-osias* to OIr. *-e* with nonpalatalized preceding consonant is perfectly regular. Methodologically it is of course desirable to try to find a common prototype for synonyms in closely related languages that look somewhat similar and have not yet received a satisfactory explanation. OIr. *asse* and MW *hawd* do very likely have a common source, but so far I have not been able to see what it is and what differences in components and phonologic history account for their divergences.

One detail: in the comparative *haws*, the *s* of the superlative has indeed spread, as Pedersen thought (§ 43, end). This is because the positive in **-s-yo-s* and the comparative in **-s-yūs* would have given exactly the same result in Welsh. In general the British languages got out of this kind of difficulty by using (originally augmentative?) adjectives in **-akko- *-okko-* in lieu of the old comparatives, but here the pattern that 'to an irregular superlative corresponds a comparative like it except for lacking *-af*' intervened, leading to *haws* beside *hawsaf*.

48. For *tressam nessam messam* there are three problems: the etymologies, the Irish comparatives *tressa nessa* (*nesso*) *messa*, and the exact relation to Osco-Umbrian *nessimo-* (*messimo-*).

On the etymologies of *tressam* and *messam* I have nothing to offer other than a strong suspicion that the latter has nothing to do with PIE **medhyo-* 'middle'. An original **miss-isŋmo-* > **missamo* > OIr. *messam* is phonologically perfectly in order and would permit a connection with Gmc. *missa-* 'wrongly', as favored by Kuryłowicz (1964:231) and considered possible by Pokorny (1954:715); but Lat. *pessimus* is also an attractive cognate, if one is willing to operate with tabuistic alteration of the initial in one language or the other.

49. For *ness-* the traditional connection with Skt. *na(d)h-* 'bind' seems very unlikely semantically, and still worse the connection with Lat. *nōdus* 'knot', E *net* etc. favored by Pokorny (1954:758–9). The obvious and reasonable connection is with Vedic *nédīyas-*, Av. *nazdyah-* 'nearer', Vedic *nédiṣṭha-*, Av. *nazdišta-* 'nearest' (Fick 1890:275): cf. von Planta (1892:378); Benediktsson (1960:242). But in that case there is a small phonologic difficulty, since the **nesd(H)-* indicated by Indo-Iranian would probably not become Osco-Umbrian *ness-*, and certainly would become OIr. **net-* and W **neth-* (cf. OIr. *net*, W *nyth* 'nest'). Celtic and Osco-Umbrian *ness-* can, however, come from **nets-*—indeed, this may be what is meant by the Gaulish graph ÐÐ—which can be related to the Indo-Iranian form by metathesis. Since PIE had *sd*, *st*, and *ts*, but no **ds*, the metathesis would have to be of **nesd-* (with a rather rare cluster) to **nets-* (with a cluster common in the prehistory of Celtic, Italic, and Germanic), not vice versa.

50. For Irish *tressa nessa* (*-o*) *messa* we must first note the other irregular Irish comparatives in *-a*: *letha* 'broader' (MW *llet*), *óa* 'minor' (MW *ieu* 'younger'), *máa má móa* (beside *máo móo móu mó*) 'more, bigger' (OW *moi*, MW *moe*, *mwy*), *lia* 'more numerous' (W **llwy*, needed to explain the replacement of *moe* by *mwy* in Middle Welsh: Sommer 1900:237), *sia* 'longer' (W *hwy*), and modern dialectal *ramha* beside *raimhe*, 'thicker', which may indicate that the OIr. form was **rema* (*raimhe* can hardly be old, since a **remyūs* should have become OIr. **rimu*: Thurneysen 1946:47, 104–5).[39]

51. Of these *lia* and *sia* can be explained as having the zero grade **-is* of the comparative suffix: PIE **pleEi-s*, **seE-is* become **pleys*, **seys*, Proto-Celtic **lēs*, **sēs*, OIr. *lía*, *sía*, with diphthongal *ía* from **ē* not followed by a palatalized consonant. Exactly the same forms gave W *hwy*, **llwy* (**plēyos*, favored by Szemerényi 1966a:n41, would have given OIr. **lí*, W **llidd*). Thurneysen (1946:236, 37) doubts that final **ē* became *ía* in Old Irish, but his countercases are not decisive. *-té* 'that he go' is part of a paradigm with regular alternation between *é* and *ía*, including a 3rd sg. absolute *téis*; *é* 'he' is probably from **es* (: OHG *er*), and *é* 'they' probably does not correspond directly to MW *wy*, which lacks a plausible explanation anyhow (MCo., MBr. *y*, *i* can be PC **īs* from masc. pl. **eyes*, corresponding to Go. *eis* like Brit. *tri* '3' to OIc. *þrír*; OIr. *é* may then be the neuter, PC **eā* : Lat. *ea*, Go. *ija*). On the other hand, the equation of OIr. *cia* 'who' with W *pwy* seems impeccable.

52. But *lia sia* ought to have been monosyllables, and their occasional disyllabic value in poetry, for which the only example I have readily at hand is *bid lia turim a chath*, *Scéla Mucce Meic Dathó* § 3.15, is more likely to have been due to the model of *tressa* : *tressam* etc. than vice versa, as suggested by Sommer (1900:238).

53. *Máa* is probably merely a plene writing for *má* from Prim. Ir. **māas*, PC **māos*, PIE **meA-yos*, with the full grade originally regular in the nominative accusative singular neuter, and so not usable as a source for *-a* elsewhere (Thurneysen 1946:237 is unrealistic), while *mó* is from Arch. Ir. **máu*, Prim. Ir. **māūs* (= PC), PIE **meA-yōs*, the nom. sg. masc. fem. form that ordinarily was the only survivor of the PIE comparative paradigm in Irish and British.[40] The graphs *máo* and *móu* are probably new-formed combinations of *má* and *mó* with the regular comparative ending *-u*, not pre-stages of *mó*. *Móa* is certainly *mó* with *-a* added, while *móo* is probably just a plene spelling of *mó*.

54. We are thus left with *tressa nessa messa letha óa *rema*, whose *-a* may have gotten some support from *sia lia má(a)*, but can hardly be explained by them alone.

Kuryłowicz (1964:231) has suggested that *-a* spread from *messa*, which was originally the feminine (and neuter) plural of an *o*-stem adjective that was used in comparative function. But since the *-yos-* comparatives had become fossilized in nom. sg. masc. fem. *-(i)u*, this adjective became fossilized too, picking the feminine plural *messa* because it had a 'redundant *-a*'. Once the pattern *olc* 'bad' *messa* 'worse' was established, *-a* spread to most

of the other irregular comparatives, so that e.g. **leithiu, *óu* were replaced by *letha, óa*.

This construction is not very convincing. It does not seem reasonable that the *o*-stem adjective **mess* would have generalized its marked feminine plural rather than its unmarked singular **mess*, which functioned in all genders (and as neuter plural as well); and *ferr* 'better', *laigiu* (etc.) 'less' (MW *llei*, PC **lagyūs*), which Kuryłowicz cites as evidence for the 'morphological nature of *-a*', suggest rather that *-a* enjoyed little if any morphologic extension beyond the forms where it was phonologically justified. Thus it is worthwhile to make a try at explaining *tressa* etc. as regular developments of the regular Celtic comparative formation.

55. It is noteworthy that Welsh *trech nes llet* lack the *i*-affection that the usual Celtic suffix **-yūs* would cause, and that appears in *llei* : Ir. *laigiu* (*ieu* is silent on this point). This has generally been taken as evidence that *trech* etc. are not old comparatives at all, or that they were formed with the zero-grade suffix *-is* that we met in *sia* and *lia*. But again, is it not more reasonable to see whether *trech* and *tressa* can be regular developments from the same comparative prototype?

56. Of these six problematic *-a*-comparatives, the two with clear Indo-European etymologies come from roots ending in laryngeal: *letha* is from **pletH-* and *óa* is from **yewH-*. For the former cf. Vedic *pṛthú- pṛthivī́ prathiná*, Gk. *Plátaia, platamón*, W *Llydaw* 'Brittany' *llydan* 'broad'.[41] For the latter the evidence is weaker. Those who believe in Edgerton's Law will attach no importance to the stem **yuw-on-* rather than **iw-on-* of Lat. *juvenis*, Skt. *yúvān-*, and I attach no importance to it because I believe that to a root **yew-* the zero grade would regularly appear as **yu-*, not **iw-*. That the weak cases of Skt. *yúvān-* have *yū́n-*, not **yun-*, is suggestive, but not decisive, since the analogy of paradigms with *uv-V* : *ū-C* from **uH* could have played a role. The acute of Li. *jáunas*, whose *-áu-* Osthoff (1910:302) suggested came from the comparative, is open to more than one interpretation. More significant is the *-ḱo-* extension **yuwn̥-ḱó-* underlying Skt. *yuvaśá-*, Lat. *juvencus*, OIr. *oac*, W *ieuanc*: if the PIE inflection of the underlying *n*-stem was acc. *yuwon-m̥*, gen. *yun-os* etc., the *-kó-* extension should have been **yun-kó-*, with a syllable structure comparable to *yungénti* 'they join'. That in fact the form is *yuwn̥ḱó-* indicates that even if **yew-*, not **yewH-* is the correct root, nevertheless already in PIE *yuwon-* was being treated as if it were **yuHon-*, and if this could happen in forming a *ko-* extension, it could lead to remodeling of the comparative as well.[42] Neither **plet(H)-* nor **yew(H)-* has an indubitable reflex that would not admit a PIE laryngeal.

57. PIE comparatives to these roots would be **plétH-yos-*, **yéwH-yos-*, with *H* syllabic between consonants and resonants which are themselves adjacent to vowels. In Celtic, where interconsonantal **H* gives *a* in all syllables, these would first become **letayūs*, **yowayūs*, and then, with a loss of intervocalic **y* which I believe has to be posited for Proto-Celtic,[43] **letaūs*, **yowaūs*. Now, while disyllables like **māūs* may have remained uncontracted down through the fifth century A.D., these three-syllable

words, accented on the initial both in PIE and by the probable Common Celtic initial stress accent, quite naturally contracted the unaccented vowel cluster *aū* to a diphthong **aų*. In British the resulting **-aus*, like all final syllables, was lost; and since it did not contain one of the sounds that cause *i*-affection, we get quite regularly MW *llet* (which only accidentally looks like the noun *llet* 'breadth' from **pletH-os* or so), *ieu*.

58. In Irish **-aus* became Prim. Ir. **-ōs*, OIr, *-o*, *-a*; cf. the genitive singular of the *u*-stems, Ogam CUNAGUSOS etc., OIr. *Conguso*, *Congusa*. That *-o* is preserved only once in the comparative, *nessoassanesso* 'nearer and nearer' (Wb 12b34a), is probably due mainly to influence from the superlatives in *-am*, which favored a phonetic spelling over the archaizing one more common in the *u*-stems. For the change to *-a*, compare *-cúala* 'you heard' from **kuklowas* or the like; here even the diphthong **-ow* left after the apocope of **-as* has become *-a*.

59. Thus OIr. *letha* 'broader' is perfectly regular from PIE **plétH-yōs*, and *óa* can well be regular from **yéwH-yōs*. To suppose that the latter might have contracted to **ó* at the stage **oo* is to introduce a gratuitous complication.

For the other four comparatives in *-a* a laryngeal root is at best feebly indicated. The etymology of Ir. *remor*, W *rhef* 'thick' is unknown, but the acc. sg. fem. *remair* (Joynt 1944:44) suggests a Prim. Ir. **remaren*, since **remren* ought to have given **reimir*. But of course analogic reshaping in such a paradigm is all too easy. W *rhefr* 'anus', cited by Stokes (1894:233), is semantically too remote to be serious evidence for a Proto-Celtic **remro-*. We are thus left with at least the possibility of a pre-Celtic **(p)remH-*.

60. For *nessa* there is no evidence one way or the other. The probable connection with Skt. *nédīyas-*, Av. *nazdyah-* (§ 49) is no evidence against a laryngeal, since the popular explanation of these from **ne-sd-* 'sit close' is not likely. To set up a PIE preverb **ne-* 'near, by' on the basis of this one word and Gmc. *nēhw-* 'near' is not sound method and does not really clear up the Germanic word; and roots, even if compounded, are regularly in full grade before *-yos-*; e.g. Vedic *práti-cyavīyas-*.

For *messa* neither of the two very dubious possible connections mentioned above in § 48 offers any support for a laryngeal. Gmc. *missa-* is probably the past participle of a root which may have been **meytH-*, i.e. **mitH-to-*, although Germanic itself indicates rather a simple **mit-* because **mitəto-* would not syncopate until well after $t+t > t^st$; in neither case would it be possible to get a Celtic **messaus* or **missaus*. And a laryngeal in *pessimus*, say *pedH-isṃmo-*, would play hob with any attempt to account for *peior*.

The internal evidence of Celtic itself indicates that *tressa* is from a root without laryngeal, since OIr. *trén*, W *tren* imply PC **treksno-* 'strong' which cannot very well be from a root **treksH-*. But Ir. *tressa*, W *trech*, not **treissiu*, **trych*, must come from somewhere. The simplest seems to be to suppose that either *nessa* or *messa* is a regular form, which served as model, either in Proto-Celtic on the pattern **-essamos* : **-essaus* = **treksamos* : **treksaus*,

or later on the patterns Ir. *-essam* : *-essa* = *tressam* : *tressa*, W *-esaf* : *-es* = *trechaf* : *trech*.

61. Traces of special outcomes for comparatives to laryngeal roots can be found elsewhere. In Sanskrit the *-yos-* comparative ordinarily appears as *-īyas-*; aside from *jyā́-yas-* from **ǵyéH-yos-* and *bhū́-yas-* from **bhúH-yos-*, the simple *-yas-* is limited to a few forms in the Rigveda. *-īyas-* has been explained as having the *-i-* that replaces other adjective suffixes in composition, of the type *ṛj-ú-* 'straight', *ṛj-i-pyá* 'falcon' (*straight flier), *ṛj-ī-yas-* 'straighter' (Wackernagel 1905:61, Wackernagel and Debrunner 1954:444, Kuryłowicz 1964:232). But the comparative ordinarily entails full grade of the root while the compositional *i*-stems have zero—e.g. *tuv-i-* : *távīyas-*, *śuc-i-* : *śócīyas-*—and it is not easy to see why the SUFFIX *-yas-* should be treated like a compositional element. It seems better to suppose with Brugmann (1906:552) that *-īyas* started in roots ending in laryngeal, e.g. *tárī-yas-* < **térH-yos-*, *dávī-yas-* < **déwH-yos-*, *śréyas-* < **śráyiyas-* < **ǵréyH-yos-*. This explains at once why the longer suffix is not to be found in Iranian (Bartholomae 1895:109; Debrunner (1936:128) is not convincing): there **H* was lost in medial syllables.

The Greek comparatives in *-ion-/-io(s)-* of course cannot have their *i* from a laryngeal. But *areíōn* 'better' beside *ár-istos* 'best' *are-tḗ* 'excellence' *aré-skō* 'please' is evidently from **AérE-yos-* (Schulze 1892:30), which in the first instance would have become **areyos-*. Addition of the customary *-on-* extension would give **areison-*, whence, with the prehistoric loss of **s* in Greek, *areíon-*. (*Khereíōn* 'worse' beside *kheírōn* is probably analogic to its antonym *areíōn*; otherwise Szemerényi 1966a:4, 10 and notes 4 and 17.)

62. One final detail about Celtic: do OIr. *máam*, *mám*, *móam* 'most, biggest', *oam* 'least', *síam* 'longest' require preforms with a simple **-sm̥mo-* rather than **-ism̥mo-*? If not, how are they to be explained? The British forms listed in parentheses in §39 show nothing unexpected. W *ieu(h)af* and *hwyaf* are normal developments (with post-junctural treatment of **-s-*) of PC **yowisamos* and **sēsamos* from **yéwH-is-m̥mo-*, **séE-is-m̥mo-*, and *mwy(h)af* merely shows the same analogic replacement of *oe* by *wy* as in *mwy*. For Irish a firm answer is hard because we—or at least I—do not know much about the development of vowels left in hiatus by loss of **s*. *Siam* may be regular from Prim. Ir. **sēsamas* > **sēhaμah* > **sēaμa* (loss of **h* before syncope because there is no provection in the Irish superlative) > **śeaμ* (shortening of long vowel in hiatus) > *sīam* (raising of **e* in hiatus as in *sieir* 'sororem').[44] For *máam* I know no second example of **'aisV-*, but an ad hoc sequence **maihaμah* > **maᵢ̯-aμa* > **ma-ᵢ̯aμ* (*ᵢ̯* in hiatus shifts to the following syllable) > *maaμ* (the Prim. Ir. loss of syllable initial *ᵢ̯* is repeated) seems perfectly feasible, while *móam* will follow the change of positive *már* to *mór*. My best guess about the development of **yow-isamos*, Prim. Ir. **owisamas*, is **uwihaμah* > **uweaμa* > **uw̃aμ* > OIr. **uam* (cf. Osthoff 1910:284); the attested *oam* is merely adjusted to *óa* and *oac* after the pattern *tressa* : *tressam*.

63. Now for the Osco-Umbrian superlatives in *-imo-*! Besides *nessimo-* 'nearest' and the dubious **messimass** 'midmost(?)' these include at least

maimas 'maximae' (twice on the Tabula Bantina, Vetter 1953:no. 2). That *ualaemom* (ibid.) means 'best' rather than 'good' or 'advantage' is less clear; the same applies to the name variously spelt **valaimas** (3x), **valamais**, **valaimais**, **valaims** (1x each) on the 'Curse of Vibia' (Vetter no. 6); cf. Sommer (1900:217).

The meaning of Umbrian **nuvime** (Iguvine Tables IIa26) is disputed. But 'ninth', first suggested by Bücheler and hesitantly accepted by von Planta (1897:197), more confidently by Buck (1904/28:56), Brugmann (1906:226), Devoto (1940:107, 336), Pisani (1953:193), Benediktsson (1960:254), Ernout (1961:62), is altogether unlikely as a replacement of **newn̥no-*, and does not give particularly good sense. Ernout argues that **nuvis** 'nine times' in the preceding line indicates that the following **nuvime** is a form of '9'; I should say rather that the impossibility of deriving **nuvime** from '9' and the difficulty that to Lat. *noviēns* ought to correspond U ***nuvif** indicate that **nuvis** is 'novius'. And for **nuvime** itself the scholars (beginning according to von Planta (1897:207) with Newman in 1864) who derive it from the Umbrian equivalent of Lat. *novissimus* 'newest', 'latest' seem altogether in the right; these include Vetter (1953:197–198), Poultney (1959:89, 183), Hamp (1962:132), Watkins (1966:36).

64. How are these forms to be analyzed? What were their original suffixes, and how is it that all five, *nessimo-* **messimass valaimas** *maimas* **nuvime** happen to contain a sequence *-im-*?

The older scholars perceived that O *maimas* has to be explained starting from the suffix **-isn̥mo-* or **-ismo-*. But since *-sm-* was supposed to be preserved in Paelignian *prismu* and Umbrian **pusme** 'cui', **esmik** 'huic' etc., some finagling was needed to explain the loss of **s* here. Von Planta (1897: 205) supposed that while original *sm* remained in *prismu* (and **pusme** etc.), *maimas* was from **mah-is-n̥mo-*[45] through **mahizemo-* (intervocalic **s* voiced), whence **maizmo-* which then became *maimo-* by a rule that *-zm-* was simplified while *-sm-* remained. In this he was followed by Sommer (1900:217), Buck (1904/28:76), Brugmann (1906:229), Benediktsson (1960:258–259). Pisani (1953:48) seems to derive *maimas* from **mais-mo-*, without discussion of phonologic difficulties.

They also saw that O *ualaemom*, **valaimas**, if a superlative, has to be explained in essentially the same way, and that the contrast in spelling between *ualaemom* and *maimas* on the Tabula Bantina suggests that the former had a diphthong but the latter a vowel sequence *aī*, which might best be explained by their differing position in relation to the beginning of the word (von Planta 1897:206).

65. The modern scholars who accept U **nuvime** as 'novissime' seem to have lost sight of the rules of morphologic patterning that were clear to von Planta. Vetter and Hamp do not discuss the form's prehistory, Poultney is content to posit a **nowimo-*, which, as Benediktsson (1960:254n96) says of **nowĕmei*, 'lacks the necessary foundation'; and Watkins (1966:36) cites **nuvime** as evidence for an Umbrian superlative formation differing significantly from Celtic *-isamo-*.

But if O *maimo-* is from **ma-isn̥mo-*, then U **nuvime** is likewise from

**new-ism̥mo-*. (cf. von Planta 1897:206–7). (Incidentally, this rules out the possibility entertained by von Planta and Benediktsson, that **-sm-* > *-m-* is a local Bantian peculiarity.)

66. Scholars who directly equate *nessimo-* and **messimass** with OIr. *nessam messam* have been hard put to explain how **-m̥m-* could give Osco-Umbrian *-im-*. Von Planta had sense enough not to posit proto-forms of the type **nezd-tm̥mo-*, **nedh-tm̥mo-* (1897:206n1),[46] but the preforms that he did posit, **nezd-ism̥mo-* or **nedh-ism̥mo-* and **medh-ism̥mo-*, forced him to assume syncope of the **i* in order to account for the Osco-Unbrian *-ss-* of *ness-*, **mess-**. This led him to conjecture that the development of **-m̥m-* to *-im-* rather than *-em-* in these words might be due to the preceding *s*, or to the whole preceding sequence *-ess-* (1892:317–8). Sommer (1900:256) and Buck (1904/28:56) attributed the *i* entirely to the influence of the preceding vowel, which is not likely. Raising by a preceding *s*, while unheard of in my experience, is at least thinkable; but how could *e* 'hyperassimilate' an obscure vowel past itself up to the *i*-position? (Recall that the general Latin change of unaccented **e* and **a* in open syllables to *i* did not operate in Osco-Umbrian).

Brugmann (1897:410) saw the difficulty and mentioned the same possibilities as von Planta, but ventured no conclusion. Götze (1923:96, 105) posited a PIE **nedh-temo-* and was then concerned with finding a way to explain why this had not syncopated to Osco-Umbrian **nesmo-*; the inadequacy of his solution (Benediktsson 1960:254) only underlines the hopelessness of trying to make Osc. *nessimo-* and OIr. *nessam* jibe precisely as **nedh-tm̥mo-*. Kent (1946:§305n5) suggests that Oscan did syncopate, and then developed *i* by anaptyxis; this is contradicted by *posmom*, which did not suffer any anaptyxis. Poultney (1959) and Ernout (1961) do not discuss the phonology. Benediktsson (1960:255) attributes lack of syncope to generalization from the nom. sg. masc. **nessims*, **messims*, where syncope in the third syllable prevented syncope in the second, and points out another difficulty: in the carefully written inscription von Planta 134a (Vetter 81), where open *i* is regularly written **í**, 'nearest' is spelled **nessimas**. He explained this by supposing ad hoc that the vowel of **-em-* merged not with the inherited short **i*, which regularly gives Oscan **í**, but with the short close *i* which is believed to have developed from **i̯* under certain conditions.

67. The correct solution should by now be obvious. Just as *maimas* and **nuvime** are from **ma-ism̥mo-* and **new-ism̥mo-*, so *nessimo-* is from the **ness-ism̥mo-* that in §44 was found to be underlying OIr. *nessam* and W *nesaf*. All three show a common sound change of *-ism̥mo-* to *-īmo-*. This explains at once the writing with **i** instead of **í**: the vowel is long. If **messimass** is indeed a superlative, and is not a miswriting for **nessimas(s)**, then it too comes from **mess-ism̥mo-* or the like.

68. There is thus in fact nothing that requires an Italo-Celtic superlative suffix **-sm̥mo-* distinct from the well-established **-tm̥mo-* and **is-m̥mo-*, and no need to posit **-tm̥mo-* where it does not belong.

Some details remain to be mentioned. These superlatives support the

opinion of Götze (1923:80) that in four-syllable words a short penultimate was regularly syncopated,[47] against the contrary opinion of Benediktsson (1960:208) that syncope in the second syllable preceded syncope in the third. Since neither Götze nor Benediktsson has numerous forms of clear etymology to support his view, these superlatives are important evidence in elucidating the rules of Italic syncope.

69. There is also the problem of exactly how the **s* disappeared. Paelignian *prismu* can be explained simply as an archaism, like Praenestine *losna* 'moon' beside Lat. *lūna*. But about the supposed preservation of internal PIE **-sm-* in Osco-Umbrian, that led von Planta to posit the complicated developments outlined above in §64, I have grave doubts. The positive evidence for preservation consists exactly of Umbrian **esmik** (Ia28, 31) **pusme** (IIa40) *esmei* (VIa5, 18) *esme* (VIb55; the tablet actually has FSME, with one stroke missing), which are regularly taken as datives and locative of pronominal stems *po-* 'who' and *e-* 'it, this' corresponding more or less precisely to Skt. dat. *kásmai, asmaí*, loc. *asmín*. As collateral evidence scholars cite several words in which initial and internal **sn* are supposed to have been preserved, of which only U **ahesnes** 'brazen' seems tolerably sure in both meaning and etymology.

70. But it is intrinsically surprising that intervocalic **s* should have been voiced to [z] in Italic generally without **s* between vowel and voiced consonant undergoing the same assimilation. Initial **sn- *sm- *sl-* may well have remained untouched in Osco-Umbrian, but I can think of no language in which intervocalic **s* underwent serious phonetic change without **s* between vowel and resonant being similarly affected. It would be worthwhile to take a closer look at U **pusme esmik esmei** *esme* **ahesnes**. I lack the time and skill for this; but in trying to work my way a little into the jungle of Umbrian philology, I am impressed by how little of what is commonly believed about that language and the purport of the Iguvine Tablets is really irrefutably founded.

71. None of the interpretations I have seen for the sentence that contains **pusme** make much sense. It occurs toward the end of the description of the puppy sacrifice, after a long series of imperatives with no expressed subject, but presumably describing actions to be done by the **arfertur** mentioned in line 16. Then there comes 'he whose duty it is shall distribute the *erus*' (Poultney 1959:188). It seems most unlikely that an assistant should have been specified in this curious way, and unthinkable that the expression should refer to the **arfertur** himself, who has required no identification for the preceding 24 lines. But as Poultney remarks (ibid., commentary) it is the presumably certain interpretation of **pusme** as 'to whom' that has determined scholars' attempts to translate the whole sentence for over a century. I cannot begin to offer a correct interpretation—every word in the sentence bristles with problems—but Umbrianists might consider trying some other meaning for **pusme**; say, 'finally', deriving it from **postm̥mēd* like O *posmom*?

72. *esme pople* (VIb55) pretty clearly means 'in this people', correlating

with *ehe esu poplu* 'out of this people' in the preceding line. *esmei* (VIa5, 16), if rightly understood at all, means 'for this'. **esmik** (Ia28, 31) is mostly taken as 'to it, upon it'; if that interpretation is right at all, 'to this, upon this' is equally possible, if not even a bit preferable, and in fact is adopted by Bottiglioni (1954:126, 263, 264).

But the simple pronominal stem *e-* does not mean 'this' elsewhere in Osco-Umbrian, or anywhere in Western Indo-European for that matter. It is either a simple anaphoric (Go. *is*, *imma*, OIr. *é*, OCS *jego* etc.), or at best a weak demonstrative referring to what precedes: O **ídík terúm** 'id territorium', or not much stronger than a definite article: **íúk tríbarakkiuf pam núvlanús tríbarakat tuset** 'the building which the Nolans may build'. It therefore seems necessary to take *esme(i)* and perhaps also **esmik** out of the paradigm of O *izic* U **erek** 'is' and assign it to the paradigm of O **eksuk** U *esu* '(ab) hoc', a complex demonstrative whose cases aside from nom. and acc. have in Oscan a stem *ekso-* and in Umbrian a stem *es(s)o-*. In that case, *-sm-* here too is not an inherited PIE cluster, but the result of syncope of **ekso(s)m-*.

73. Whatever the correct interpretation of these forms, they cannot seriously weaken the clear evidence of O *maimas* and the almost equally clear testimony of U **nuvime** and OU *nessimo-*. Either we posit for Osco-Umbrian a superlative formant *-imo-* (*-īmo-*?) unknown to the rest of Indo-European and of inexplicable origin, or we admit a development of **-isŋmo-* to Osco-Umbrian *-īmo-*. The choice is not hard.

74. It turns out then that if we ruthlessly hack away the innovations that have piled up during centuries of independent change, we find that all the Italic and Celtic superlatives that do not belong to the *-(t)ero-/-(t)ŋmo-* set go back to a single prototype, **-is-ŋmo-*. Most of the attested forms can be derived from this by strict application of regular sound laws, or at least a process no more refractory than the haplology of **-sisamo-* to *-samo-* in Celtic. Even the Latin *minimus* and *-issimus*, which are not fully accounted for, nevertheless can be safely considered late and specifically Latin deformations of **-isŋmo-*, not relics predating any conceivable period of Italo-Celtic unity.

75. As far as I know, it has never been suggested that this **is-ŋmo-* is a shared archaism of Italic and Celtic that was replaced by **-is-to-* in Germanic, Greek, and Indo-Iranian. Not that such a distribution is impossible of itself—witness the *r*-endings of the medio-passive—but rather because it is unlikely that *-isŋmo-* with its clear formal similarity to the *-(t)ŋmo-* superlatives should have been replaced by an *-isto-* with a *-to-* that occurred nowhere else in the comparative/superlative system.

Whether *-isŋmo-* is, as generally believed, a replacement of *-isto-* on the analogy of *-(t)ŋmo-* or whether *-isŋmo-* and *-isto-* are independent extensions of *-yos-/-is-* in different areas of late Proto-Indo-European (as suggested by Brugmann 1903:10), the fact remains that *-isŋmo-* is an INNOVATION, SHARED by all forms of Italic and Celtic, and EXCLUSIVE to them.

76. There are several other such exclusively shared morphologic innovations of Italic and Celtic, all dating, as far as I can judge, from the 'late Proto' Indo-European period, very roughly 4000 years ago.

77. One of these is the thematic optative in *ā*, contrasting with **oy* in Germanic, Balto-Slavic, Greek, Indo-Iranian, and Tocharian: Lat. *faciat*, O **fakiiad**, U **façia**, OIr. *-bera* (W *oedd* 'erat'?) vs. Go. *bairai*, OCS *beri*, Li. *te-nešiẽ*, Gk. *phéroi*, Skt. *bháret*, Toch. B *klyauṣi* 'would hear'. This interpretation of the Italo-Celtic so-called '*ā*-subjunctive' was first seen by Trubetzkoy (1926), and worked out further by Benveniste (1951). Here again it seems unlikely that the Italic and Celtic form is an archaism that has been replaced by *-oy-* in the rest of Indo-European. True, the latter consists transparently of thematic vowel *-o-* plus the zero grade of the athematic optative suffix *-yē-/-ī-*, while the analysis of *-ā-* is opaque (my suggestion, Cowgill 1965:160n43, should not be taken very seriously). But this is not very likely, given the very wide distribution of *-oy-* and the lack of any relic of a modal *ā* outside of Italic and Celtic.[48] Whether *ā* has replaced *oy* or whether the two are independent ways of extending the optative from roots to thematic formations, as is perhaps more likely, this morpheme, like superlative *-isṃmo-*, is an exclusively shared innovation of Italic and Celtic.

78. As Watkins (1966:41–3) has rightly pointed out, the modal systems of historic Italic and Celtic are very different. But it is important to see that the differences are entirely the result of secondary divergence, later than the creation of the *ā*-form itself. Thus in Italic the *ā*-optative continued to coexist with the athematic *-yē-/-ī-*, while in Celtic this other formation died out and instead the *ā*-modal got paired with formations coming from the PIE subjunctive which, so far as they occur in Latin, are worked into the future system; e.g. OIr. *-bé*, agreeing in meaning with Lat. *sit* but in form with Lat. *-bit*, or OIr. *-sius* 'sedeam', agreeing in formation with Lat. *faxō*. (My views on the prehistory of the Irish *s*-subjunctive differ from those of Watkins 1962.)

While Irish and Latin form *ā*-optatives to **eye/o*-presents in the same way (Watkins 1966:41–2 does not consider the evidence of OIr. forms like *du'loigther* 'that it be forgiven', Ml 58d2), the Italic languages have all found different ways to cope with the homonymy of indicative and modal stems in the *āye/o*-verbs that was tolerated by Irish: Lat. *amet*, *O deiuaid*, U *portaia*, although probably containing a common element **-ē-*, show different ways of combining this with the verb stem *-ā-* and the usual mode suffix *-ā-*.

While in Latin the mode suffixes became restricted to occurrence with stems that also form indicative presents and perfects, and there form a stately array of four 'subjunctives' plus future, imperfect, future perfect, and pluperfect indicatives, in Celtic the existence of modals whose indicatives had died out or never existed leads to the 'subjunctive' having a stem of its own, alongside the present, future, and preterit; and instead of four subjunctives made by using two formants and two stems, we get only the

creation of a 'past subjunctive' by adding to the subjunctive the same endings that when added to a present indicative produce an 'imperfect'.

79. But when all this adventitious superstructure is sheared off, and the old items lost by one branch or the other are put back, we get a common Italo-Celtic mood structure remarkably close to what can be reconstructed on the basis of Homeric Greek and Vedic Sanskrit, except precisely for having *ā* in place of *oy*: short vowel *e/o*-subjunctives formed to roots (Lat. *erit*, OIr. *-bé*) and probably to *s*-aorists (these acquired independent mobility, with the result that in Irish almost no verbs have both *s*-subjunctive and *s*-preterit) and long vowel *ē/ō*-subjunctives formed to thematic stems (Lat. *dīcet*); *yē/ī*-optatives formed to roots (Lat. *sit*, *velit*) and *ā*-optatives formed to thematic(ized) stems (Lat. *ferat*, OIr. *-bera*). Furthermore we find that none of the later accretions—Italic 'imperfect subjunctive' *-sē-* and the like—have been plausibly related to developments outside Italic and Celtic: once again we have the pattern of an early common innovation overlaid by centuries of divergence, but divergence along independent lines, not agreeing with the development in any neighbor dialect.

80. We know now that the medio-passive *r*-endings as such are a retention, not an innovation. But the use of *r* as a voice marker pure and simple IS an innovation. In Tocharian and Anatolian the *r* serves to distinguish 'primary' from 'secondary' mediopassive endings, i.e. it is primarily a 'tense' marker (like *-i* in the active), and as a voice marker it is only auxiliary to the fundamental morpheme *o(/e?)*. (Basic on these endings is Pedersen 1938:100–8.) Thus Hitt. *eš-a-ri* 'sits', *eš-a-t* 'sat', Toch. B *paika t-ä-r* 'will write', *paikā-t-e* 'wrote'. In Italic and Celtic *r* is no longer restricted to 'primary' tenses, but occurs also in forms descended from the PIE optative, which took secondary endings: witness Lat. *ferātur*, U **emantur**, OIr. *-berthar*. (The lack of a special deponent form in the Irish imperfect etc. and the lack of *r* in general in the Celtic imperfect passive are clues that may some day be of use in discovering the true origins of these puzzling formations.) *-r* (more precisely **-(o)r*) has thus become a voice marker and nothing else in Italic and Celtic.[49]

81. In addition, Italic and Celtic have innovated together in creating a 1st pl. medio-passive **-mor* to replace the PIE **me-dhə*, **-mos-dhə* that is attested in Gk. *-me(s)tha*, Skt. *-mahi*, Hitt. *-wašta*, Toch. (B) *-mte* (and possibly Go. *-nda*). The model for this was probably the thematic 1st sg., where the falling together of **oA* and **oAo* as **ō* had entailed a new analysis of act. **-o-A* and medio-passive **-o-A-o-r* (person marker *A*, voice marker *-o-*, tense marker *-r*) as *-o-:* and *-o-:-r*, with */:/* functioning as person marker and *r* perforce carrying the whole burden of the voice distinction. Although a proportion *-ō* : *-ōr* = *-mos* : *-mor* is not exact, Italic and Celtic seem to have had the (secondary) **-mo* as well as the (primary) **-mos* that is preserved in Latin. At least, *nidanchumachtig* 'non dominamur' (Wb 14c41) and *nidan chosmili* 'we are not like' (Wb 14d37) point that way. The innovation would then date from a time when primary and secondary endings still coexisted in the first person, but had already become interchangeable in value there.

82. The notorious genitive singular in *-ī* is less decisive. Without going into details, it seems likely that we can set up a common prestage for Italic and Celtic in which masculine and neuter pronouns formed their genitive in **-syo* and *o*-stem nouns and adjectives had a genitive in *-ī*. Celtic and Latin essentially preserve this, Osco-Umbrian replaced both endings with *-eis*. Faliscan tended to spread the pronoun ending to the noun (cf. Giacomelli 1963:142–4). The *-syo* of the pronouns is securely Proto-Indo-European, while a genitive in *-ī* is not found in any of the other major branches of Indo-European, nor is it very likely that the endings that do occur in them have replaced *-ī*. But *-ī* is not a sure Italo-Celtic exclusive innovation because the little-known Messapic, which is neither Italic nor Celtic, nevertheless has numerous genitives in *-ihi* which are presumably to be read *-ī*.

83. Other possible exclusively shared innovations of Italic and Celtic that have been suggested from time to time are too nebulous to be pursued here. But the four that I have discussed in this paper—assimilation of **p . . . k^w* to *k^w . . . k^w*, superlative in *-isṃmo-*, optative in *-ā-*, and *r* as a voice marker—while hardly enough to establish a real subgroup, do seem to require a time when Italic and Celtic were closer to each other than either was to any neighboring dialect of which significant material has survived. But this period of contact seems to have been broken off very early, not to be restored until the Celtic invasion of Italy at the beginning of the fourth century B.C.

84. To suppose, however, that there was an early period in which the dialects destined to become Italic and Celtic were but little if any differentiated and that this unity was broken only by the migrations of the 'Italicans' southward that eventually landed them in Italy would be too simple. For both Celtic and Italic appear to have been in contact with Germanic after having broken off effective contact with each other (Porzig 1954:123 on Celtic and Germanic, 117 on Italic and Germanic). In addition, there are a number of innovations shared by all three, Celtic, Italic, and Germanic (see Porzig 1954:123–7 for one list), of which the most significant is no doubt the development of PIE **tst* to *ss*.

85. It might seem reasonable to start out with a situation in which Celtic, Italic, and Germanic are three neighboring dialect nuclei in PIE, with Celtic at the west and in contact with no other surviving dialect except Italic, while Italic shared contacts not only with Celtic but also with (at least) Germanic, which in turn exhibits contacts of its own with other dialects to the east (and south). Then when the Italicans pull up stakes and move south, the Celts and the Germans naturally move into the vacuum from both sides and the specific Celto-Germanic innovations not shared by Italic date from this period.

86. But this would require some rather complex maneuvering. Since Italic shares no innovations exclusively with Germanic that are anywhere near as deep cutting as the innovations that it shares with Celtic, and since the innovations that it does share with Germanic appear to be somewhat later than those it shares with Celtic, the initial picture of three dialects

simply strung out in a line has to be modified to account for the fact that at the time of common Italic and Celtic innovations the Italicans apparently were not in effective contact with the Germans. Either they were geographically separated (by uninhabited territory? by some group that has since disappeared?), or they just did not have much to do with each other. Then, when the Italicans did begin to move, they did not just head south over the Alps. Rather they moved so as to pretty thoroughly break off their ties with the Celts and at the same time (give or take a century or two) establish contact with the Germans, contact that must have lasted several centuries to produce the amount of change it did. At the same time, presumably, the Celts and Germans were establishing relations of their own, in which the Italicans did not share. It was only after a few centuries of this situation that the Italicans broke away from the northern European area altogether. And where are the innovations shared by all three subgroups to be dated? Do they require a still earlier period in which the Italicans, located between Celts and Germans, did have contacts with both their neighbors?

87. Admittedly, simplicity is a criterion of very low priority in determining what went on in prehistoric migrations. The population movements we know of from historical times are too complex for simplicity to outweigh any other kind of consideration in determining the more plausible of two competing hypotheses about prehistoric population movements. But in this case, where I know absolutely nothing about the archaeological evidence for migrations in north central Europe before the first millennium B.C., I would suggest for consideration another set of prehistoric events the virtue of which is precisely that they are simpler than what was sketched in §86.[50]

88. Let us rather start with a situation where Italo-Celtic is a single dialect nucleus, presumably at the extreme west of the Indo-European area. This is at a time when the language is phonologically essentially unchanged from Proto-Indo-European, except that probably laryngeals have already merged with zero, length, or *a*. Morphologically the language is also very close to PIE, but does have such distinctive features as superlative in *-isṃmo-*, optative in *-ā-*, and 1st pl. medio-passive *-mor*.

The Germans at this point are not in contact with this group at all, but probably off somewhere to the east (the case forms in *-m-*, shared with Balto-Slavic against the rest of Indo-European, might be developing about this time). But now there is a Germanic migration, right into the middle of the Italo-Celtic area, which at one and the same time cuts the area into two distinct groups, Celts and Italicans, and establishes contact between both groups and the Germans. Not only the common Italo-Germanic and Celto-Germanic innovations would date from this time, but also the common Italo-Celto-Germanic innovations. The last act, as before, would be the migration south of the Italicans, leaving the Celts and Germans in sole possession (aside from tribes that may have become extinct without leaving linguistic records) of North Central Europe.

89. The movements just sketched are simpler than those sketched before not in that they are fewer—I have merely substituted one Germanic and one Italic migration for two Italic migrations—but in that they do not require such precise statement of position (there are fewer distinctive features, as it were). It is no longer necessary to posit a period when Italicans and Germans were adjacent but not in active contact. And it is no longer necessary to visualize a complex movement on the part of the Italicans such that they broke contact with the Celts and at the same time established contact with the Germans. Rather we have the simplest movement imaginable, a straight thrust, but performed by the Germans rather than the ancestors of the Romans and Osco-Umbrians.

90. I do not know how well either of the above schemes fits the archaeological evidence about population movements in prehistoric Europe. But at least I think I have shown that the superlatives of Italic and Celtic are derivable from a single common set of ancestral forms not found anywhere else in Indo-European.

Notes

* In preparing the published version of this paper, I have been helped not only by the comments of my fellow scholars assembled in Philadelphia—among whom Hamp, Kuryłowicz, Tovar, Watkins, and Wyatt are especially to be thanked—but also by comments from Professors C. E. Bazell, O. Szemerényi, and R. Ward.

[1] But I believe Watkins (1966:34) and Hoenigswald (1966:7) are mistaken in thinking it important that Proto-Italic has four voiceless stops *p t k k*^{w} while Proto-Celtic has only three, *t k k*^{w}. When the assimilation occurred, PIE initial **p* before vowel cannot yet have merged with zero or anything else in pre-Celtic—cf. Bolelli (1962: 101–4), a reference which I owe to Szemerényi—and I do not see that its merger here with **k*^{w} has any significant similarity to its merger with plain **k* before *t* and *s*. Surely the most likely development is that **p* continued to exist for some time, maybe a thousand years or more, as a distinct structure point in pre-Celtic after its merger with **k*^{w} in the special environment **p ... k*^{w}.

The canonical derivation of OIr. *deec* '-teen' from **dwei-penk*^{w}- 'two fives', cited by Watkins as evidence for the nonuniversality of the assimilation, involves too many difficulties to be maintained. *dwei-* is not the PIE combining form of 'two', the unpalatalized final *-c* is inexplicable starting from **penk*^{w}*e*, and it is hard to imagine why the early Celts should have replaced the pivotal number **dekan* '10' with a compound precisely for use in combination with a digit and then their descendants preserved the idiom unchanged down to historical Old Irish. Welsh *deunaw* '18', while a parallel, only points up the difficulties: 18 is not a pivotal number, the expected form is not simple, but a phrase, *tri ar bymtheg*, and *deunaw* contains the regular Welsh words for '2' and '9' without forcing us to invoke any peculiar prehistoric forms or dubious sound laws. Hertz's derivation (1955:69) from **dekṃk*^{w}*e* or **dekṃk* 'and 10', with dissimilatory loss of the *-ḱ-* is thinkable, but requires us to divorce the formation from that of '21' etc. Reichenkron's derivation (1958:169) is most unlikely (I am indebted to Szemerényi for these two references).

Rather, *deec* should be derived from a genitive of '10', parallel to *fichet* 'twenty-', *trichot* 'thirty-', etc. This would be **dekan-os*, formed by adding the genitive ending *-os* to the otherwise indeclinable **dekan*. From this OIr. *deec* can be derived through a metathesized **deankos;* to be sure, the reason for the metathesis is not clear to me now.

Szemerényi (letter of 18.7.66) has suggested Proto-Celtic **dekant-os* as the starting form. But there is no other relic in Irish of an inflected **dekṃt-* beside the normal

deich, so that probably *deëc* is a Celtic innovation made to fit a new fashion in counting, not a relic. *deich* comes from **dekm̥*, not **dekm̥t*, and this *t*-less form of '10' must have been regular in pre-Celtic already before the change of **-m* to **-n*—otherwise the *-m-* of *decametos* etc. would be hard to explain—so that **dekan-os* is more likely as the shape of a Celtic innovation than is **dekant-os*. Furthermore **dekantos* > *deëc* involves not only a metathesis curiously different from the retention of **-kVnt-* in *fichet* etc., but also an ad hoc loss of **t;* while if we start from **dekanos*, *CV-CV-nos*, the change to **deankos*, *CV-Vn-Cos*, would have been supported by its greater approximation to the syllable structure *CV-CVn-Cos* of **wikantos* and **trīkontos*.

[2] This is, after all, nearly the same as what Meillet said, *Introduction à l'étude comparative des langues indo-européennes* 8 (1937) 67: 'Italique et celtique représentent un même dialecte indo-européen et ont dû se séparer de l'ensemble de l'indo-européen vers le même temps, à une date reculée, sans doute avant le germanique et le grec. Puis ils ont divergé, de sorte qu'ils diffèrent à beaucoup d'égards, malgré l'identité probable de l'état initial.' Here I would disagree only with the view that Italo-Celtic was already differentiated from the rest of Indo-European at a time when Germanic and Greek were still part of a homogeneous and nearly uniform dialect area.

[3] But forms that seem to point to ablaut variants *-toro-*, *-tro-* are probably secondary developments.

[4] A separate category 'preposition' did not yet exist.

[5] Toch. A *lyutār* 'excessively, more' has been mentioned as a possible example (Sieg, Siegling, Schulze 1931:32). To be sure, I would expect PIE **-tero-* to give Toch. A *-cär*, but cf. the middle participle *-māṃ*, which is certainly related to Gk. *-menos*.

[6] In their earliest attested forms Anatolian, Armenian, Tocharian, and Albanian lack a morphologic comparative; whether any of them ever possessed one and if so what its shape was can of course not be known.

[7] Both show loss of a lenited dental in an unaccented word, as in *lae* 'day' and *(s)ón* 'that' beside *laithe*, *sodain*. *arN* further exhibits dissimilation from *narN* (attested restressed as *nár* 'of us two'); and *farN*, *barN* (phonemically /bhar/) exhibits the voicing of obstruents in proclitics that was regularly noted in Old Irish times only in the case of *d-* from **t-*. The restressed *sar* mentioned by Thurneysen (1946:279) has apparently arisen by mistakenly taking **far* as containing the initial of *sí*, *sib* 'ye' lenited.

[8] To be sure, I cannot now say exactly how PIE **usmé* became Celtic **swe-*. It is at least clear that *sethar* is not directly comparable to Lat. *voster*, *vester*.

[9] On Baltic **pīrmas* 'first' see §§24, 25. In view of the complete lack of a morphological superlative elsewhere in Balto-Slavic, the Lithuanian formation in *-iáusias* is pretty clearly a special Lithuanian creation, not a transformation of one of the types found elsewhere in Indo-European. But it is interesting as a parallel to them, since, like *-is-to-* and *-is-ṃmo-*, it is based on an extension of the comparative, in this case in the shape that shows up in the Lithuanian adverbs in *-iaũs*. Kuryłowicz (1964:234) suggests that its *-ia-* is originally the same element that appears in the Balto-Slavic 'definite' adjective, so that *geriáus-ia-s* is 'LE meilleur' in origin; cf. Vaillant (1955: XXII). This is very attractive, and if correct, is a good parallel to the analysis for PIE **-is-to-* that I propose in § 17.

[10] Cf. the loss of superlatives in Romance, Greek, and Irish within historical times.

[11] On the function of these Germanic forms see Szemerényi (1960:1–21). A discussion of the phonemic status of PIE **ṃ* and **m* is outside the scope of this paper. But in the formations discussed here the two patently fail to obey the rules for resonants laid down by Edgerton (1943:86); *-tṃmo-* has no variant *-tmo-* after light syllables, *-mo-* and *-ṃmo-* alternate according to whether the preceding phoneme is a vowel or a consonant, not according to the weight of the preceding syllable, and the ordinal **sebdm-o-* 'seventh' (n. 18) has *-mo-* after a heavy syllable.

[12] But U *śimo* 'zurück' can hardly be a form of a superlative corresponding to Lat. *citimus* 'closest to here' only without *-t-*. Lat. *fermē* 'almost, for the most part' is not an unambiguous superlative; its exact relation to its synonym *ferē* (cf. Thurneysen

1916) remains to be cleared up. Other apparent Latin examples of *-mo-* are discussed in §§ 33–6, *īmus*, O **imaden** in § 40.

Of the possible Celtic or pre-Celtic Indo-European superlatives in *-amo-* from the Iberian peninsula cited by Schmoll (1959:44), the personal names *Medamus*, *Medama* agree well enough in form with Skt. *madhyamá* 'midmost', Go. *miduma* 'middle', but it is hard to believe that family planning was well enough advanced in ancient Iberia for parents to be able to name a child 'Midmost' with any confidence that the name would turn out accurate. Similarly *paramus* 'high plateau', although close enough in form to Skt. *paramá* 'most distant, highest, best', is not superlative in sense. The personal name *Clutamus* and the word *arimo*, *arimom* cannot well be superlatives because the suffix *-(ŋ)mo-* is not appropriate to the stems **ḱluto-* and **ar(ə)-*.

[13] So von Planta (1897:205) and Thibau (1964:39); cf. DeWitt (1937:70). This is better than the semantic development posited by Leumann (1923:26), and both are much better than the usual derivation from *ops*, as if one were to say 'wealthmost'. Despite the argumentation of Leumann, *loc. cit.*, I think it unlikely that *maritimus* 'dwelling by the sea, marine', *fīnitimus* 'bordering', and *lēgitimus* 'lawful' are faded superlatives; I have no useful conjecture about the true source of their *-timus*.

[14] But it is not clear to me why **últiumam** and *hondomu* have kept (or restored) a vowel before *m* that is lacking in **pustm**[, *posmom;* Benediktsson (1960:255) is not convincing. It is also not a matter of course that Osco-Umbrian should have created replacements for the inherited **n̥dhero-*, **n̥dhŋmo-* by adding *-tero-* and *-tŋmo-* to the noun stem **ǵhom-* 'earth'. Perhaps (following a private suggestion of Szemerényi) the mediating form was a locative **ǵhom(i)* 'on the ground, down(ward)'.

[15] But Celtiberian *sintamo*, *sintamom* and the Gaulish personal name *Sentamos* cannot well be superlatives to *sen-* 'old', partly because the stem for 'old' is *seno-* in both Celtic and Proto-Indo-European, partly because *-tŋmo-* is not the right suffix.

[16] I am indebted to Konstantin Reichardt for calling this reference to my attention.

[17] For the benefit of readers not used to Indo-Europeanist graphemics, I should explain that the *m* of *-ŋmo-* is not a separate phoneme but is the automatic glide which is presumed to have existed between a syllabic resonant and a following vowel.

[18] It could be argued that *hébdomos* was not extended by *t* because this extension applied only to forms in *-ŋmo-*, and when **dekŋmo-* was extended to **dekŋto-*, '7th' was still **sebdmo-*. But **dek̑ŋmo-* is a very unlikely PIE form anyhow. Its violation of Edgerton's Law by having a syllabic resonant before vowel after a light syllable is not in itself a serious objection; but **sebdm-o-*, the form for '7th' required by Greek and Balto-Slavic (cf. Sommer 1950:17 n1; Szemerényi's attempt to derive OCS *sedmŭ* from **septŋmo-* [1960a:6–7] is unconvincing; on p. 110 he admits a Balto-Slavic **septmos*) and by the hypothetical argument just proposed, violates Edgerton's Law in the opposite sense, by having a nonsyllabic resonant after a heavy syllable. Rather than assume that PIE '10th' and '7th' both contradicted Edgerton's Law and in opposite directions, it seems more reasonable to suppose that if **septŋ* became **septm-* before *-o-* at the time when ordinals were being formed, in disregard of the supposed law, then a fortiori **dek̑ŋ* plus *-o-* would appear as **dek̑m-o-*. Since in fact 'PIE' **dek̑ŋmo-* is posited entirely on the evidence of of Celtic, Italic, and Indo-Iranian, languages in which **sebdmo-* was replaced by **septŋmo-* (Lat. *septimus*, Gaul. *sextametos*, Skt. (YV+) *saptamá-*), it follows that if there was a pre-Greek word for '10th' without *t*, this can only have been **dekmos* (**degmos*??), and it remains inexplicable why this should have been remodeled to *dékatos* while **sebdmos* merely developed regularly to **hebdmos*, *hébdomos*.

[19] Since writing this, I have heard my wife's five-year-old daughter use the ordinal *sevenest*.

[20] List of all forms in Müller (1962:147–8).

[21] The *þ* of *tēoþa* compared to the *d* of Go. *taihunda*, OIc. *tíonde*, OHG *zehanto* is evidently from **feurþan-* '4th' and **ahtauþan-* '8th'; cf. OHG *fiordo*, *ahtodo* (Go. *ahtudin* shows Thurneysen's Law).

[22] This is no place to go into the manifold ways in which Old High German was

influenced by Latin and Romance, especially in its vocabulary, for everything even a little bit intellectual (*schreiben, lesen*!), but I can't resist reminding the reader that the same Western Franks to whom I impute the replacement of **zweinzuguto* by *zweinzugosto* were the ones who replaced their native words for 'lord', *truhtin, fraho* (> *frō*) with *hēr(i)ro*, lit. 'elder' (< 'grayer'), a calque on Gallo-Romance *sendra*. Icel. *tuttugasti* would then have a history similar to that of *herra*.

[23] Lat. *quotumus* 'the howmanieth' beside *quotus* 'id.', has long been recognized as another example of this type, e.g. Sommer (1914:457). Even more primitive in appearance than Greek are Toch. B *ikante* '20th', A *taryākiñci** '30th' etc., which look like direct descendants of the **wīḱṃt-o-*, **trīḱṃt-o-* posited by Szemerényi (1960a:90). But in view of the instability of ordinals above '10th', the late attestation of Tocharian, and the fact that descriptively *ikante* and *taryākiñci* contain a suffixal *-t-* vis-à-vis the cardinals B *ikāṃ*, A *taryāk*, it seems best not to claim a greater antiquity for them than for the forms of Sanskrit, Greek, and Latin.

I should add that in line with the shapes of the cardinals I would posit PIE forms **wīḱṃti-to-* and **trīḱomt*to-* etc., reflected more or less faithfully in Skt. *viṃśati-tama-*, *triṃśat-ta-má-* (for transposition of nasal in the latter see Szemerényi 1960a:55) and Gk. *triakostós*. Gk. *eikostós* has borrowed the ending of the higher ordinals. Lat. *-cēnsimus* is **-konssemos* with the vocalism of **wikentitemos* (just as *-gintā* has *i* from *vīgintī*), while late Av. *-sąstəma-*, *sastəma-* reflect scribal confusion of the proper outcomes of **-śantstama-* and of **-śastama-* for **śatitama-*.

[24] Whether the immediate outcome of prevocalic **ṃm* in the dialects ancestral to Latin was *em* or *am* is a difficult problem. I incline to accept the evidence of *tenuis* etc. over that of *manēo* etc., and so have written **em* in this paper; inserting a stage **am* would not make any substantial difference.

[25] Schwyzer (1939:503–4) is rather misleading. But the Arcadians did create a *pempotos* (IG 5:2.33), obviously after **tetrotos* '4th', with Arcado-Cypriot *ro* from **r̥*, as in Myc. *qe-to-ro-*, Thess. *petro-* 'tetra-' < **kʷetwr̥-*. Attested Arc. *tetortos* shows the same metathesis as Attic *tétartos*. In Cowgill (1966:90) I failed to perceive that **tetrotos*, **petrotos* must have played a major rôle in shaping Arc., Lesb. *dekotos*, *deko*, Lesb. *enotos*, Myc. *e-ne-wo-*, Arc. *hekoton*. *Dúo* probably has little or nothing to do with these forms, and *eíkosi*, *-kósioi* of Attic and other dialects are a separate innovation, based on *-konta*.

[26] I.e. another significant and early Italo-Celtic innovation, shared only with the noncontiguous Indo-Iranian. The alternation of *ši-ip-ta-mi-ia* with VII-*mi-ia* in Hittite texts as the name of some kind of liquid is far from proving that Hittite had an ordinal **šeptama-*.

[27] Since a form of '6th' made by simply adding *-o-* to the cardinal **sweḱs* is attested nowhere else and since *sextametos* etc. can hardly have been formed until after **sweksetos*, the *suexos* of La Graufesenque must be a mistake, as Thurneysen (1946:250) suggests, despite Szemerényi (1960a:78).

[28] Cf. Arabic *'awwalu* 'first', fem. *'ūlā*, an elative in shape.

[29] This was probably dissimilated from **pró-tato-s*, as suggested by Lohmann (1933:345 n1), rather than built directly as **pró-ato-s* replacing *pró-mo-s* with an *-ato-* imported from *húp-ato-s*, *púm-ato-s* and the like.

[30] In a paper read at the University of Durham in March 1967, I have suggested that *prâtos* dates from a pre-Greek layer of contraction in which *oa* contracted to *ā*, of the type discussed by Wackernagel, *Das Dehnungsgesetz der griechischen Composita* (1889), and of which *stratāgós* from **stṛto-agos* is a familiar example. *Prôtos* would be a restored or unchanged **próatos* which contracted later by the same rules that apply to vowel sequences resulting from loss of **s* and **y*. This is the solution implied by Bechtel, *Die griechischen Dialekte* 1.236 and 2.51, 97, 171.

This explanation of *prâtos* removes the main obstacle to the view that combinations of PIE **r̥*, *l̥*, *n̥*, *m̥* plus laryngeal had three phonologically regular outcomes before consonant in Greek, *Rē*, *Rā*, *Rō*, depending on the timbre of the laryngeal. I am therefore now (June 1969) in substantial agreement with R. S. P. Beekes, *The*

development of the Proto-Indo-European laryngeals in Greek (1969) 216, 218–20, that in Greek $R\bar{V}$ from $*\underset{\circ}{R}H$ the timbre of the long vowel depends on the identity of the laryngeal involved.

But in Italic and Celtic these combinations regularly result in *Rā*, even if the laryngeal was *E* or *O*. Cf. Lat. *(g)nātus* from $*ǵn̥E$-*tó-s* and *strātus* from $*stṛO$-*tó-s*, and OIr. *lán*, W *llawn* 'full' from $*pl̥E$-*nó-s*. In the other major subgroups of Indo-European, reflexes of $*\underset{\circ}{R}H$ clearly do not coincide with reflexes of **Rā* (**ReA*) in Germanic, Balto-Slavic, Indo-Iranian, and Anatolian. For Tocharian, Armenian, and Albanian I am not sure what the phonologically regular outcomes of $*\underset{\circ}{R}H$ combinations were, but the possible examples that I know of generally do not coincide with expected outcomes of **Rā* in those subgroups. It follows that the development of $*\underset{\circ}{R}H$ to *Rā*, regardless of the laryngeal involved, is an important early Italo-Celtic exclusively shared innovation which should be added to the four listed in §83.

[31] Slightly otherwise Thomas et al. (1958:800, 810).

[32] Hiersche (1964:141–4) argues convincingly for aspiration by the preceding sibilant.

[33] Götze's rule (1923:80) that a short third syllable of a four-syllable word was syncopated seems to work better in Osco-Umbrian than in Latin (cf. § 68). Professor Ralph Ward informs me that he has independently concluded that in Latin four-syllable words both of whose medial syllables were short regularly syncopated the second, not the third. (Cf. Leumann 1928:92–3.)

[34] Götze (1923) does not discuss syncope of uncompounded five-syllable words, but syncope of either the second or third syllable of four-syllable words is compatible with **superisemos* > *suprēmus*. In the former case the rule must be extended to include alternate short internal syllables starting with the second and moving forward; in the latter it must include alternate short internal syllables starting with the penult and moving backward.

Whether *-is-* > *-es-* preceded or followed syncope is not immediately evident. But since pressure for creation of *-issimus* (§ 37) would have sharply increased after syncope, the fact that this is not **-essimus* suggests that the change in vowel timbre was later than syncope, and that the first change was to **suprismos*, not **superesemos*.

[35] Iliad 10.224–6: 'Two men going along together, the one thinks of something before the other, so that there may be advantage. But a single man even if he thinks of something, nevertheless his mind is *brássōn* and his thinking is insubstantial.' The meaning 'shorter', while conceivable, hardly imposes itself.

[36] On maturer reflection, I see that *ipsimus* can very well have developed by haplology from *ipsissimus*, in exactly the same way that forms like *sūmpse* (attested from Naevius onward) developed in Latin beside *sūmpsisse* etc., and very similarly to the Celtic words discussed in §§39–47. My doubts about the meaning are excessive; while the meaning of *ipsimus* does not require it to be etymologically superlative, nothing rules out the possibility of such an origin.

[37] Cf. von Planta (1897:205 n1).

[38] Morris Jones (1913:243–9) posits comparatives with **-son* from **-isōn*.

[39] Distribution of forms in Ireland in Wagner (1958:Map 227) 'fatter'.

[40] Of the two sources considered for OW *moi* by Jackson (1953:357–8) **māūs* is better —although **mais* (<**meA-is*), as in Oscan and Gothic, is also thinkable.

[41] Szemerényi (1964:157) is too skeptical. Conceivably Celtiberian *Letaiśama* is not a writing for **Letisama* (Schmoll 1959:45), but reflects analogic extension of *a* from preconsonantal position.

[42] On OE *geogop* etc. see Winter (1965:198).

[43] The *dd* of W *trydydd* etc. comes from what in Proto-Celtic was a nonphonemic glide in /tritios/. While the loss of intervocalic **y* is shared with Italic and is earlier than either the change of **ē* to *ī* or the loss of **p*, Greek has it too, and so it is not diagnostic for Italo-Celtic subgrouping. Watkins (1958:97) does not note that Arch. Ir. *tre* '3' is attested only as neuter and as genitive (Thurneysen 1946:242, Greene and Quin 1948:303), where it continues PC **triā* and **trion*.

[44] This raising did not occur in the sequence **ee*. Hence OIr. *deec* '-teen' < Prim. Ir. **deeggas* < **deankos* (note 1 above); *deed-* 'lazy' < **desed-*; and *tee* 'hot' < Prim. Ir. **teēs* < PC **teents* or **teants* (Thurneysen 1946: 50, 228). The raising was earlier than loss of intervocalic **w*, as shown by OIr. *dëu* 'deos', *dëacht* 'deity' from Prim. Ir. **dēwūs*, **dēwaktā*.

[45] The *-h-* is of course antiquated. Osc. *maimas, mais*, U **mestru** must come from the **meA-* of Germanic and Celtic; the difficulties inherent in deriving them from the *mag-* of Latin are well put by Walde and Hofmann (1954:14).

[46] The popular interpretation of O **nistrus** as 'propiores' not only requires a misspelling of **i** for **e** and a rather dubious semantic usage, but also a morphologic monstrosity.

[47] As far as Osco-Umbrian is concerned. But *maximus* etc., as observed in note 33, suggest that in Latin the antepenult was the first to go.

[48] The Tocharian *ā*-subjunctive is in fact simply the nonpast inflection of verb bases ending in *ā*: Toch. A *kälp-n-ā-tär* 'obtains' : *kälpā-tär* 'will obtain' : *kälpā-t* 'obtained', pp. *kälpo* < **kälpā-u* = Skt. *gṛbh-ṇ-ī-te* 'seizes' : nonexistent **gṛbhī-te* : *agrabhī-t* 'seized', pp. *gṛbhī-ta*. See now my article, Ablaut, accent, and umlaut in the Tocharian subjunctive, in *Studies in historical linguistics in honor of George Sherman Lane* (= *University of North Carolina Studies in the Germanic Languages and Literatures*, number 58 [1967]) 171–81.

[49] It is neither certain that Venetic is an Italic language (see Polomé 1966) nor that Venetic *doto, donasto* 'gave', *fagsto* 'made', which certainly are not middle or passive in sense, are middle in form. For more on the structure of deponent and mediopassive verb forms in Proto-Indo-European, Italic, and Celtic, see now my article, The first person singular medio-passive of Indo-Iranian in *Pratidānam, Indian, Iranian and Indo-European studies presented to Franciscus Bernardus Jacobus Kuiper on his sixtieth birthday* (1969) 24–31.

[50] Cf. Dyen (1956) for the theoretical background of this and the following paragraphs.

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Proto-Indo-European Culture: The Kurgan Culture during the Fifth, Fourth, and Third Millennia B.C.

Marija Gimbutas

Post-World War II developments in archaeology in general and the breath-taking pace of excavations in the Soviet Union and the satellite countries have changed many of the old views on "die indogermanische Urheimat." Chronological and geographical gaps gradually are being filled and archaeologists can present some facts with some certainty. The existence of Indo-European homelands advocated by linguists for more than 100 years is no longer an abstraction; results achieved by archaeological research make it possible to visualize the homelands, at a certain time and place, as a historical reality.

Ten years ago (in 1956) at the International Congress of Ethnological Sciences in Pennsylvania I gave a paper on "Cultural change in Europe in 2000 B.C." and ventured to show a map of the expansion of the Kurgan Pit-grave culture to Europe from the Ukraine and south Russia, beyond the Dnieper River. At the time carbon-14 dates were nonexistent for dating cultures in this part of Europe, chronological gaps were obscuring the whole picture of cultural development, and the culture north of the Black Sea was insufficiently explored. However, it was clear that only a new force from beyond the Dnieper could have caused the upheaval and disintegration of the long-lasting and flourishing Chalcolithic cultures of Europe and their contemporaries: the Cucuteni-Tripolye, Gumelniţa-Salcuţa, Butmir, Vinča, Lengyel, Tiszapolgár-Bodrogkeresztúr, Funnel-necked Beaker; and primitive Forest cultures in northeastern Europe: the Pit-marked Pottery culture in central Russia and the Comb-marked Pottery culture in the East Baltic area. Instead of a colorful mosaic formed by agricultural cultures, elements of totally different character spread over a wide area, suggesting a transition to a predominantly pastoral economy and patriarchal society. Burial rites, religious symbols, ornamental motifs, and equipment that originated in the Eurasian steppe region became widespread.

The hypothesis of the cultural change in Europe as well as in the Near East caused by the invasion of hordes of pastoralists from the North Caucasian-lower Volga steppe area continues to be supported by new archaeological data and new dating possibilities. Of particular importance

were discoveries of stratified village and hill-fort sites of the Kurgan culture between the lower Dnieper and lower Volga; the discovery of another neolithic culture north of the Black Sea, called Dnieper-Donets, coexisting with the Kurgan culture throughout the fifth millennium B.C.; the recognition of Kurgan materials in Cucuteni-Tripolye in the Ukraine and Moldavia sites and their synchronization, which is of great value in establishing the chronology of the Kurgan advancement westward; the disruption of the flourishing Gumelniţa-Salcuţa culture of Bulgaria and Rumania and Tiszapolgár-Bodrogkeresztúr in the east Hungarian Plain by the Kurgan people indicated by typical Kurgan graves and other elements of their culture, as well as by the carbon-14 dates obtained for Kurgan graves and settlements.[1]

Constantly accumulating archaeological discoveries have effectively eliminated the earlier theories of Indo-European homelands in central or northern Europe and in the Balkans. The Kurgan culture seems the only remaining candidate for being Proto-Indo-European: there was no other culture in the Neolithic and Chalcolithic periods which would correspond with the hypothetical mother culture of the Indo-Europeans as reconstructed with the help of common words, and there were no other great expansions and conquests affecting whole territories where earliest historic sources and a cultural continuum prove the existence of Indo-European speakers.

The acknowledgement of the advance of Kurgan cultural elements and people during the fourth and third millennia B.C. from the east to Europe and from the north across the Caucasus to the Near East permits us to direct and concentrate our attention on the area from which the movements started.

I. Terminology

The name *Kurgan culture* (the Barrow culture) was introduced by the author in 1956[2] as a broader term to replace Srednij Stog II and Pit-Grave (Russian *Yamna*), names used by Soviet scholars for the culture in the eastern Ukraine and South Russia, and Corded, Battle-Axe, Ochre-Grave, Single Grave and other names given to complexes characterized by elements of Kurgan appearance that formed in various parts of Europe after the infiltration of Kurgan elements from north of the Black Sea. The word *Kurgan* (*barrow* in Slavic and Turkic) is retained because it has appropriate connotations of eastern origins (Fig. 1).

The Kurgan culture covers Early, Middle, and Late periods of cultural development between the lower Dnieper and southern Siberia and all its synchronous manifestations outside this area. The question of continuous development throughout the fourth and third millennia B.C. is not doubted. Metal was already known, but in earlier phases was very rare. The culture was in a chalcolithic (copper-stone) stage except for its late period in the centers of metallurgy (such as the Caucasus) which should be classed as Early Bronze Age.

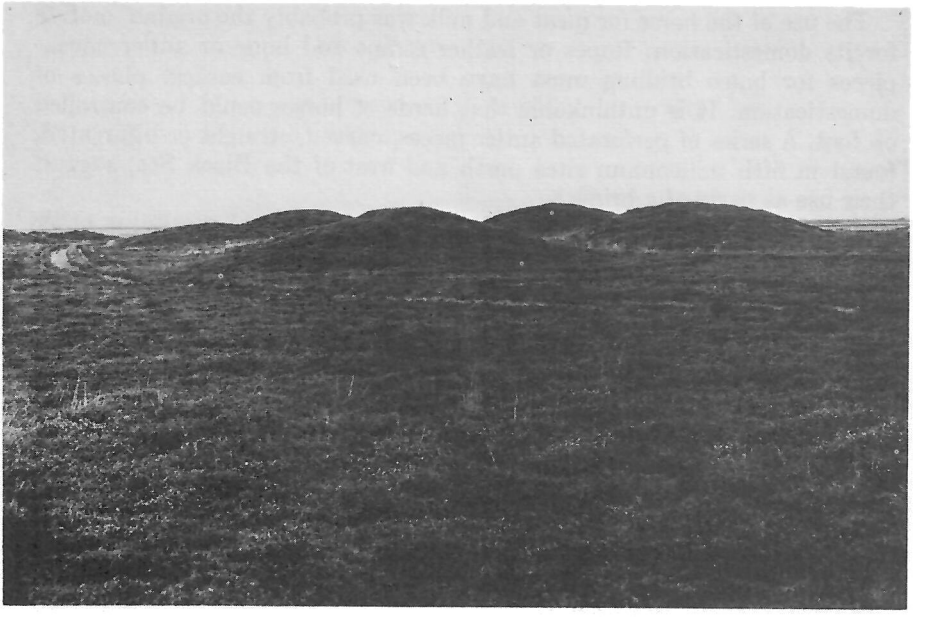

FIG. 1. The tumuli (round barrows, "kurgans") typifying the Kurgan culture during the Chalcolithic and its Bronze and Iron Age manifestations in Europe and Asia. Photo from Föhr, Western Germany.

II. General Characteristics

ECONOMY. Economy was predominantly pastoral. The principal domesticated animals—cattle, sheep/goat, horse and pig—were known throughout the Kurgan period. As yet the evidence for the initial phases of horse domestication is too thin. Long-lasting Kurgan settlements are not known. Usually they are found on dunes, and only a few yielded information confirming horse breeding. Domestic animal bone found in Early Kurgan villages yielded horse bone up to 70 percent–80 percent of the total. A village at Dereivka near Kremenchug in the lower Dnieper area showed the following percentages of domesticated animal bones: horse 68 percent, cattle 21 percent, pig 9 percent, sheep and goat 7 percent.[3] That wiid animal bones accounted for only 17 percent of the total in this village is indicative of the relative unimportance of game animals for food. In another settlement, Khutor Repin on the lower Volga, horse bones made up 80 percent of all domesticated bones.[4] Bones were discovered in rubbish pits among other remains of meals. The dating of the radiocarbon content of horse bones from the village of Dereivka at the Geophysics Laboratory of the University of California at Los Angeles (done by Reiner Protsch) gave the date of 4400 B.C. (This date is corrected according to a calibrated scale produced by radiocarbon analysis of sequential tree ring samples of known age. Thus the date is a true calendrical approximation rather than an expression in conventional radiocarbon years.)

The use of the horse for meat and milk was probably the original motive for its domestication. Ropes or leather straps and bone or antler cheek-pieces for horse bridling must have been used from earliest phases of domestication. It is unthinkable that herds of horses could be controlled on foot. A series of perforated antler pieces, curved, straight or bifurcated, found in fifth millennium sites north and west of the Black Sea, suggest their use as parts of a bridle.[5]

Flat figurines in the form of a horse's head, made of diorite or other semiprecious stone and presumably used as sceptres having magical protective powers, have been discovered in the lower Volga region, the north Caucasus steppes, the Ukraine, Moldavia, Dobrudja, Transylvania, and southern Yugoslavia. Some were found in typical Kurgan graves.[6] All figurines so far known are very similar to each other. Some of them clearly show decoration of the horse's head by grooves or ridges around the nose and along the top of the head. This might be a representation of a bridle made of leather straps or of ropes (Fig. 2). These sculptures are the earliest pictorial representations of bridled horses. In the Kurgan sites of the third quarter of the third millennium B.C., cheek-pieces for horse bridles were already made of metal (made of a bronze/copper wire with a loop in the middle) in Transcaucasia.[7]

To identify domesticated horse bones is a difficult matter. Only very recently have animal bone specialists advocated the presence of domesticated horse with some certainty.[8] Horse was locally domesticated, and the process could have been similar to what was known in the Siberian steppes in the eighteenth century A.D.: only geldings were ridden, the main herds of horses were kept more or less wild under a stallion, control being exercised through the mares which were milked and kept hobbled near the tents.[9] Settlements usually yield both domesticated and wild horse bones. The wild horse of the Eurasian steppe, called *tarpan*, has been identified among the remains of wild animal bones and this same kind was domesticated.

The horse was in great demand by the more civilized, agricultural peoples of east central Europe and the Balkan Peninsula. The presence of the domesticated and bridled horse in the Cucuteni-Tripolye, the Gumelniţa and Tiszapolgár-Bodrogkeresztúr cultures in the fourth millennium B.C. speaks for a rapid spread of horse domestication westward. The diffusion of Kurgan elements westward seems to have been connected with the spread of horse domestication. Horse bones are known in the early Tripolye (Pre-Cucuteni) and classical Tripolye (Cucuteni A) settlements in the western Ukraine[10] and in the Tiszapolgár-Bodrogkeresztúr sites of east central Europe.[11] The Gumelniţa layer of the Photolivos mound in the Drama Plain in Macedonia excavated in 1968 by the UCLA-Sheffield University Expedition yielded a horse head figurine with a possible indication of a horse bridle (Fig. 3). Horse bones also were found in several Gumelniţa sites south of Bucharest.

The Kurgan people lived in the steppe and forest-steppe zone, but in the fifth and fourth millennia the climate was warmer and damper than at

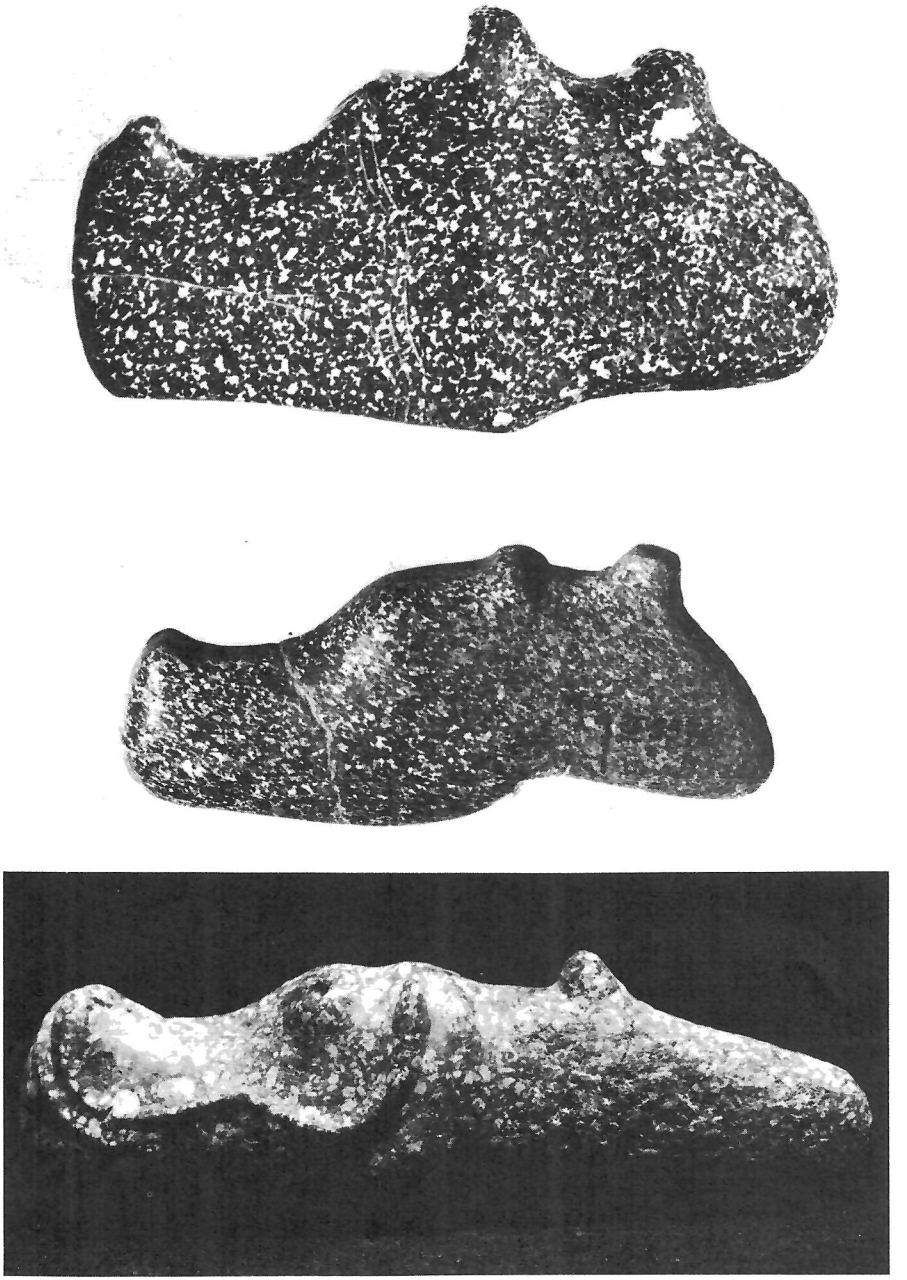

FIG. 2. Horses' heads carved of diorite, used as sceptres. Harness parts are indicated by incision or relief. *1*, Sălcuța, SW Rumania; *2*, Fedeleşeni, Moldavia; *3*, Šuplevec near Bitola, South Yugoslavia. Approximate date: end of the fifth millennium B.C.

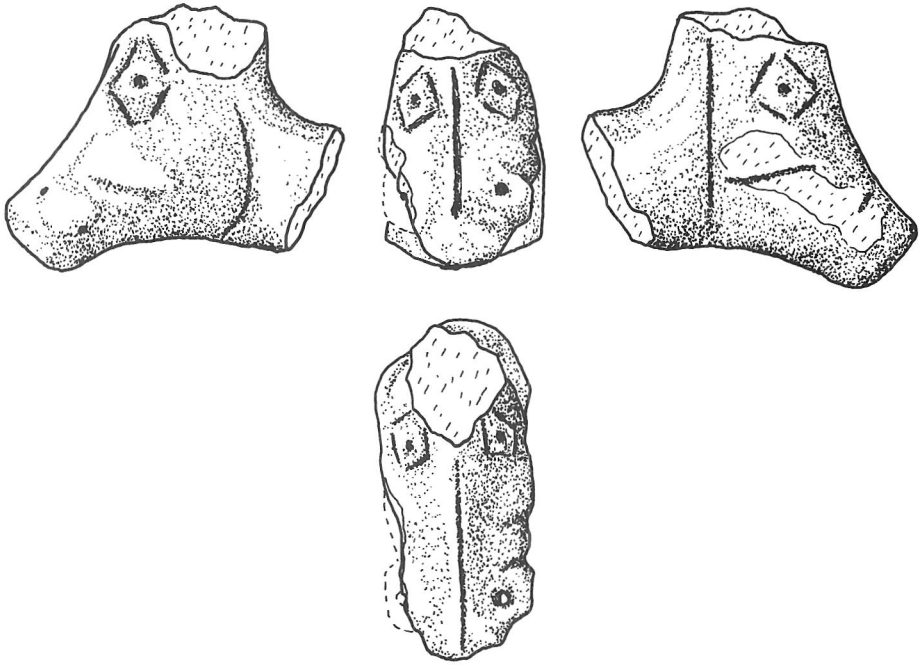

FIG. 3. Horse (?) head figurine of clay (Scale 1/1) from the Gumelnita (Dikilitash) layer of the mound of Photolivos near Drama in Greek Macedonia. UCLA-Sheffield University excavation 1968. The age: end of the fourth millennium B.C.

present and what is now the steppe zone was more forested. Mixed forests, including oak, birch, fir, beech, elder, elm, ash, aspen, apple, cherry and willow, extended along rivers and rivulets in which such forest animals as aurochs, elk, boar, wild horse, wolf, fox, beaver, squirrel, badger, hare, and roe deer were present. These are evidenced in the settlements of Mikhajlovka in the district of Kherson.

Elk antler, cattle and sheep bones, and boar tusks were amply used for tools and ornaments. One of the most typical Kurgan tools which appears in graves and settlements was a hammer-hoe made of elk antler. This could be a prototype of the later battle-axe made of stone or metal. Bone awls, chisels, and polishers are very frequent finds. Flint arrowheads were used as weapons and for hunting. Wooden bows are not preserved, but their presence is attested by engravings on stelae and stone slabs of graves. From engravings and actual traces in graves it is known that the Kurgan people carried arrows in skin quivers as did the Scythians. Flint scrapers, knives, perforators, and copper and bone awls were used for fur and leather work. The presence of bone or antler harpoons, points, fishhooks, and fish bones in Kurgan villages indicated that fishing was an important means of subsistence.

Agriculture was practiced but not highly developed. Only a few flint sickles are known from the entire Pontic region. Grindstones, pestles, saddle-querns or flat stones in settlements are evidence of agricultural activities.[12] Millet grains and melon seeds were found in Late Kurgan settlements and graves north of the Black Sea. A triangular sandstone instrument, 26.5 cm long and having a sharp edge, found in the upper layer of the Mikhajlovka settlement, may have been used as a ploughshare. Until now very little study has been done on the question of Kurgan farming north of the Black Sea. Analyses of pollen spectra in the Netherlands have shown that grain cultivation must have been of importance to the earliest Kurgan settlers in northwestern Europe. In Denmark, plough marks have been observed underneath a barrow of Kurgan type, which is proof of the use of the wooden ard.[13] Typically, the Kurgan people settled in flat grasslands, in a landscape favorable to horse and cattle grazing. When they expanded it can be seen that they were searching for pasture lands, such as the steppe area north and west of the Black Sea, Dobrudja, the Danube Plain, the East Hungarian puszta, the Kuro-Araxes Valley in Transcaucasia, and the prairie lands of Syro-Palestine.

Remains of two- and four-wheeled wooden vehicles with solid wheels of oak and their models in clay have been found in the lower Dnieper and Volga region. Disk wheels and their miniature models in clay are also known from almost all areas to which the Kurgan people spread. From what we know of the Dnieper and Volga regions these represent an open four-wheeled cart and a covered two-wheeled steppe wagon. Actual remains and clay models are known from graves. They apparently served as hearses for royal burials; miniature models of clay were deposited as symbols of real wagons. How early they appeared we do not know since such objects are preserved only in exceptional cases. Judging from the grave types of Storozhevaja Mogila in the lower Dnieper north of the Black Sea, in which wooden remains of two-wheeled carts were found (Fig. 4), the earliest vehicle may date from the Early Kurgan period, at the latest from the early part of the third millennium B.C. Solid wheels of oak from the peat-bogs of the Netherlands belong to the end of the third millennium as carbon-14 dates show,[14] and thus are roughly synchronous with Late Kurgan vehicles in south Russia and the Caucasus. There is no indication that horses were used for traction. More probably oxen were used for this purpose as they were in Transcaucasia where small figurines of yoked oxen in pairs, cast in copper and dated by carbon-14 to around 3700 B.C., are found. The vehicle and metal seem to have been transmitted from the East Anatolian-Caucasian (Kuro-Araxes) culture to north of the Caucasus range and the Black Sea. In Mesopotamia vehicles are traced back to the Uruk period—late fourth millennium B.C.

Before the Late Kurgan period metal was represented in the Pontic steppe region only by copper awls and tanged, leaf-shaped knives or small daggers. In Late Kurgan times flat shaft-hole axes appeared which, along with tanged dagger points and awls, were made on the spot (Fig. 5). The

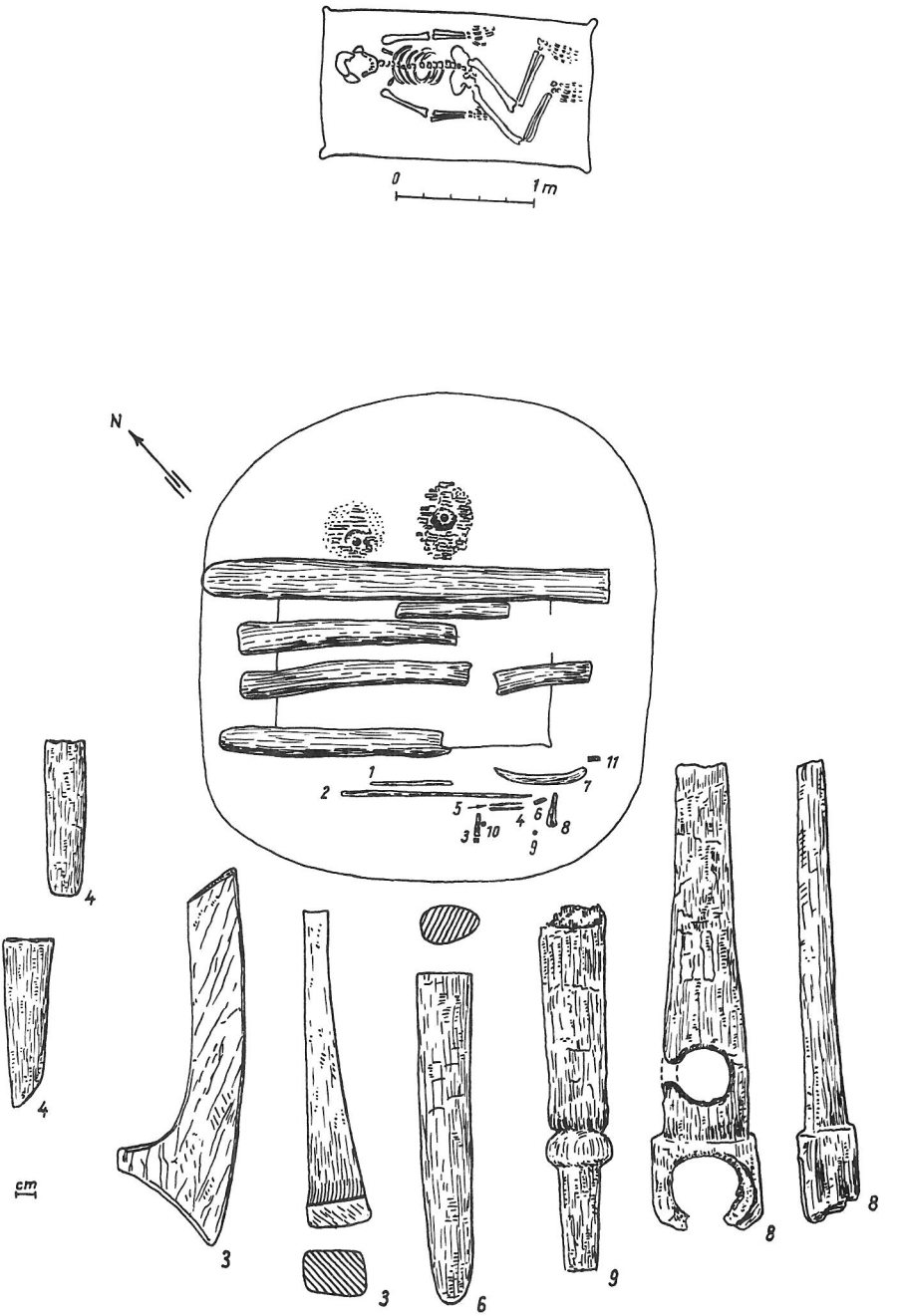

FIG. 4. Preserved wooden parts of a two-wheeled cart from the grave at Storozhevaja Mogila in the lower Dnieper area. The illustration shows the skeleton lying on its back with contracted legs (top), two solid wheels of oak and other parts as they were found (middle), and some of the small parts in detail. Tentative date: fourth millennium B.C. *After* Terenozhkin, 1951.

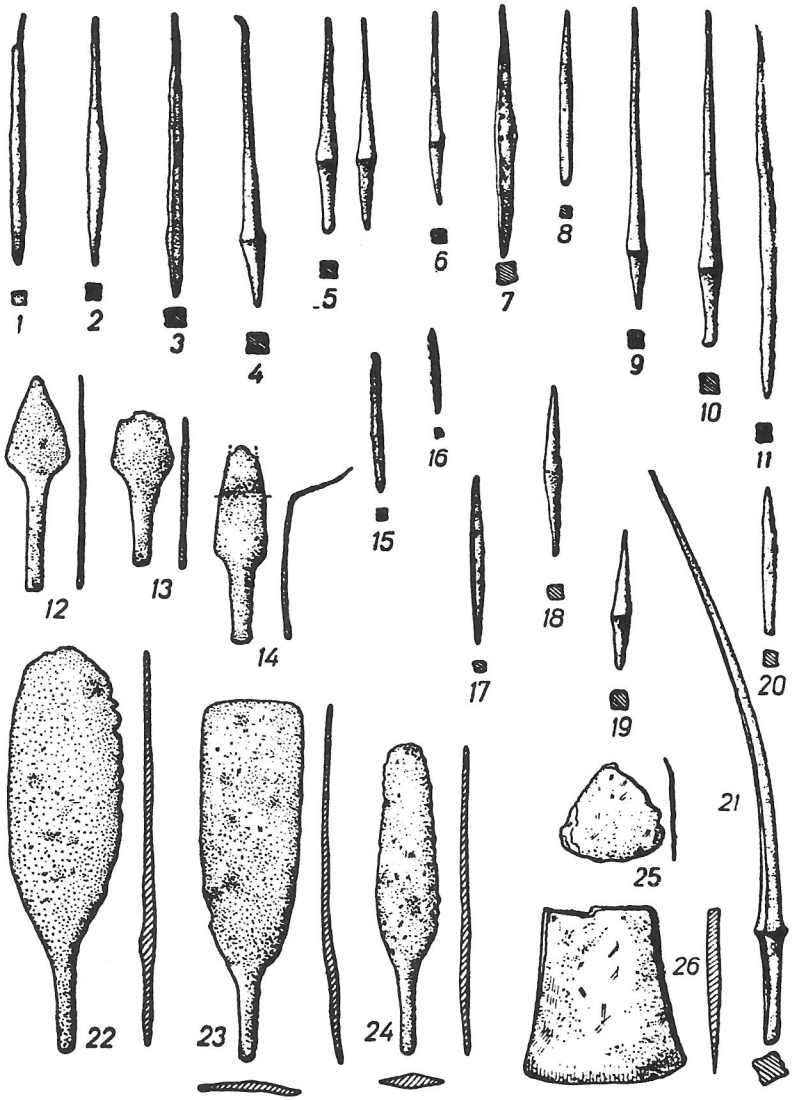

FIG. 5. Copper tools from the hill-fort of Mikhajlovka (all except three awls belong to the upper layer of the hill-fort). *1–11, 15–21*, awls; *12–14, 22–24*, daggers and knives or razor blades; *25*, a copper plate; *26*, part of a flat axe. Scale: approx. ½. *After* Lagodovska, Shaposhnikova and Makarevich, 1962.

existence of local metallurgy is proven by nozzles and crucibles. The Kurgan people in the northwestern Caucasus were acquainted with copper, silver, and gold from about the middle of the third millennium B.C. as is shown by the quantities of gold or silver vases, beads, and rings, as well as bull, goat, and lion figurines, copper axes, adzes and axe-adzes, daggers and knives,

discovered in the royal tomb of Majkop and the hoard of Staromyshastovskaja in the valley of the River Kuban.[15] These objects have parallels in the treasure horizon in Anatolia and Mesopotamia. It is certain that Anatolian and Mesopotamian metalwork extended its influence to north of the Caucasus mountains after the Kurgan invasions or raids of the Near Eastern lands.

Pottery was crude, and unpainted; clay was mixed with crushed shells and sand. Forms were extremely conservative, consisting of oval, pointed bottomed pots with necks of various sizes, apparently originally derived from basket shapes. However, flat-based dishes and beakers were used concurrently. Decoration was executed by incision or stabbing with a sharp tool which was usually triangular in cross-section. Other methods included by pit impressing, cord impressing and impressing with a stick wound with cord. (See pottery types from the two periods of the settlement of Mikhajlovka, Fig. 6.)

Social Structure. Information about social structure derives from habitation patterns and burial rites.

There were two types of settlements: simple villages and hill-forts. Small villages were located on river terraces and consisted of about ten to twenty small, rectangular, semisubterranean houses with pitched roofs supported by thin wooden posts (Fig. 7). Dimensions of houses vary from 6 × 13 meters to 10 × 16 meters. Stone-walled hearths were found (both inside and outside of houses), usually one to a house.

The fully excavated village of Dereivka in the lower Dnieper region occupied an area of 2000 square meters, whereas, for example, larger villages of the Cucuteni-Tripolye culture in Moldavia and the western Ukraine occupied an area of 15,000 square meters or even more. In some villages nearly two hundred houses were found. Hill-fort sites were placed on steep river banks, in places difficult of access, usually on promontories at the confluence of two rivers. The following are the best-known examples from the lower Dnieper and Don area: (1) Mikhajlovka, in the district of Kherson on the high bank of the Podpil'na River near its confluence with the Dnieper, is the most important stratified site with its last layer heavily fortified with massive stone walls 3 meters in height, built of about ten rows of large stones, surrounding the settlement from the inland side (Fig. 8); rectangular houses built of timber on massive stone foundations (up to 1 meter in height) had two, three, or more rooms of large dimensions (one house being 45 meters long) (Fig. 9); these houses have no resemblance to the semisubterranean village houses. (2) Skelja-Kamenolomnja, south of Dniepropetrovsk, is another impressive hill-fort, situated on a steep rocky promontory 30 meters high overlooking the Dnieper River; on three sides the cliffs made the site inaccessible, and the sloping western side was fortified with a thick stone wall; the plateau of the hilltop had an area of about 1 hectare; this hill-fort, dating from the Kurgan III phase, also included rectangular houses built on stone foundations; the site contained workshops for fabricating polished stone tools, such as battle-axes and mace-heads. (3) Liventsovka at Rostov on the Don, on a high hill sur-

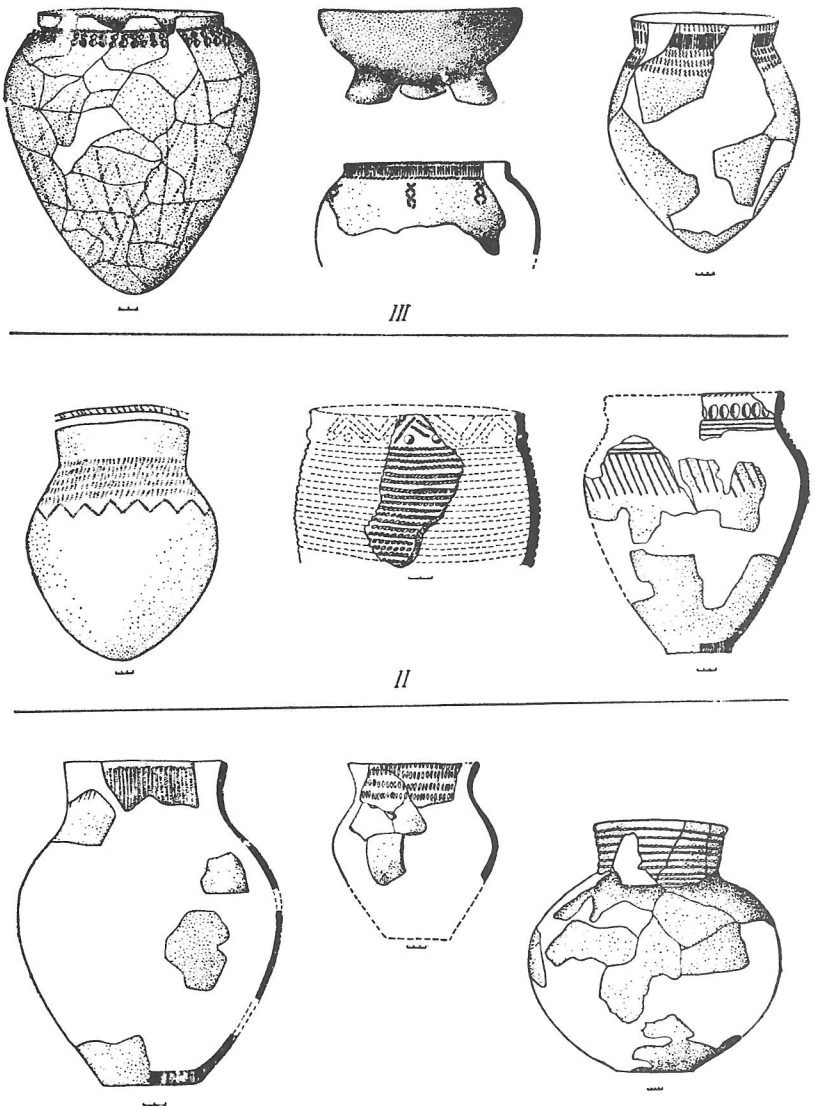

FIG. 6. Three stages of pottery development from the middle and upper layers in the settlement of Mikhajlovka. *After* Lagodovska, Shaposhnikova and Makarevich, 1962.

rounded by a massive stone wall and ditches from outside and inside the wall, excavated in 1962–65.[16] The hill-fort yielded five strata (ranging from the Majkop phase to the Bronze Age Timber-grave). The fortifications are held to be of Late Kurgan (Catacomb-grave) age.

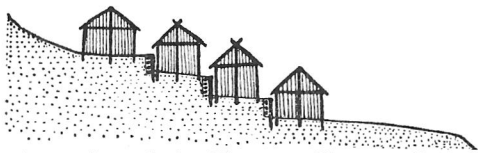

FIG. 7. A schematic section of the Kurgan village at Rzucewo on the Bay of Gdańsk (Danzig) arranged on reinforced terraces of a dune from the first half of the third millennium B.C. *After* Žurek, 1954.

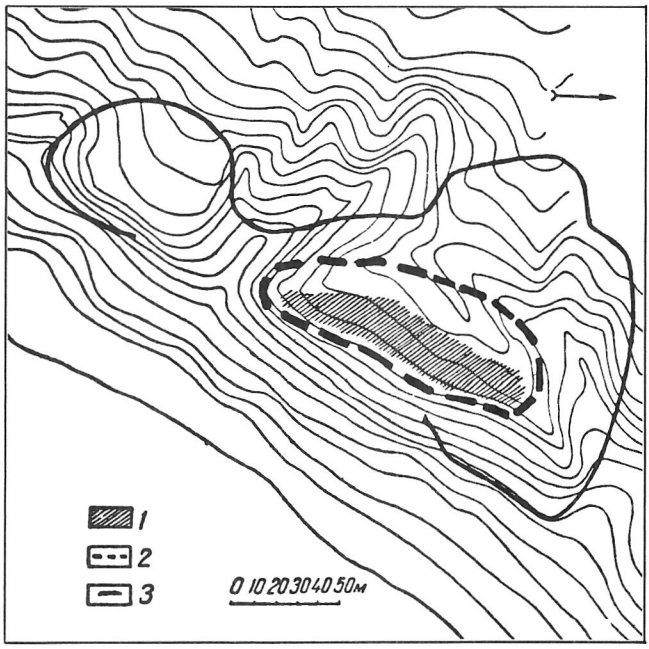

FIG. 8. The situation of the three settlements of Mikhajlovka on the small river of Pidpil'na: *1*, Mikhajlovka I; *2*, Mikhajlovka II; *3*, Mikhajlovka III. The latter occupied a large territory and was protected by huge "Cyclopean" stone walls on the inland side. The settlements range from ca. 4000 to ca. 2300 B.C. *After* Lagodovska, Shaposhnikova and Makarevich, 1962.

Other Kurgan hill-forts of the third millennium B.C. are from the Balkan Peninsula and east central Europe. A large hill-fort existed at Šuplevec, 17 km northeast of Bitola in present southern Yugoslavia not far from the Greek border. Now this impressive hill-fort is converted into a stone quarry. During the excavation of 1959 it was surrounded by massive stone walls.[17] Seventeen or eighteen houses were uncovered standing in three rows in an oval area of the hill-fort. Houses were 4 to 5 meters long and about 3 meters wide, built of vertical timber posts standing in intervals of about 1 meter. Each house contained one either square or circular hearth.

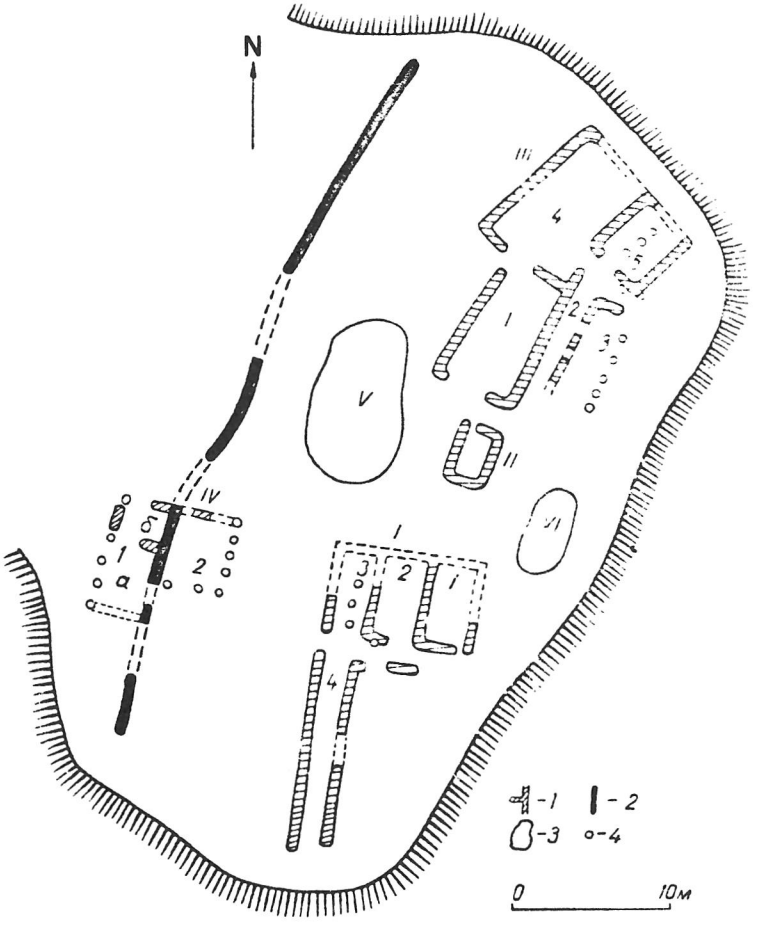

FIG. 9. Plan of the Mikhajlovka III citadel. *1*, stone foundations of houses; *2*, a Cyclopean wall from the inland side; *3*, traces of unexcavated houses; *4*, traces of holes from wooden posts. *After* Lagodovska, Shaposhnikova and Makarevich, 1962.

In East central Europe—northern Yugoslavia and western Hungary—many hill-forts are reported, among which are the famous Vučedol near Vukovar in northern Yugoslavia, on a steep hill almost inaccessible from three sides (Fig. 10), and the Nagyárpád near Pécs in southern Hungary, on which excavations were finished in 1966.[18] The latter is the most impressive of all, with fifty small houses standing in rows along the paved road leading to the top of the hill, and two large wooden houses, probably royal, on the terrace. This hill-fort could house about 250 people. The Vučedol and Nagyárpád hill-forts belong to the post-expansion times (Vučedol to ca. 3300–3000 B.C., Nagyárpád to the third quarter of the third millennium

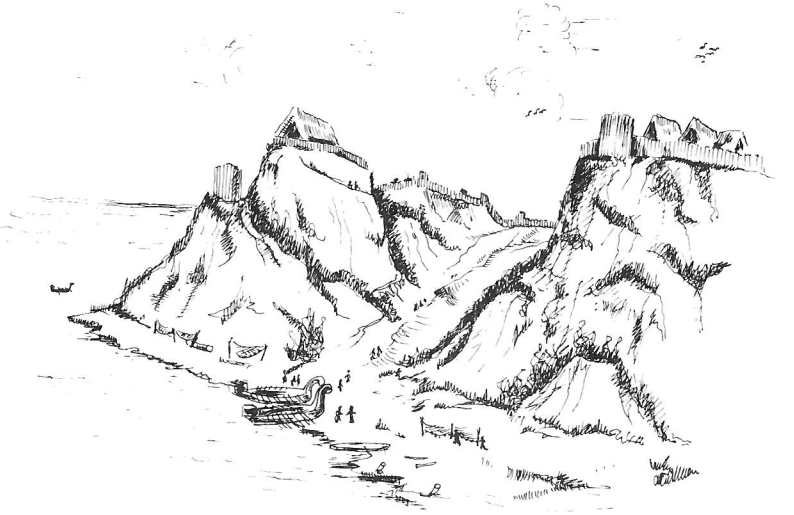

FIG. 10. The hill-fort of Vučedol near Vukovar, northern Yugoslavia. A reconstruction. *After* R. R. Schmidt, 1945. The "Baden" period ca. 3000 B.C.

B.C.). The above mentioned hill-forts can be considered tribal centers and are prototypes of the later Mycenaean Greek, Illyrian, Celtic, Baltic, Slavic, Germanic, and other Indo-European castle-hills, belonging to the chief of a tribe or his subordinates.

Members of the royal and warrior classes can be recognized in exceptionally rich graves; some of these men accumulated a fabulous amount of gold, silver, and precious stones as can be seen, for example, in the Majkop royal tomb of ca. 2500–2400 B.C. in the Kuban region of the northern Caucasus. The Majkop king lay in the largest of the three rooms of his mortuary timber house (in two others lay a male and a female corpse), and was dressed in a garment onto which gold ornaments were sown: 68 lion figures, 19 bulls and 38 rings. Close to his head were found beads and buttons of gold, silver, turquoise, and carnelian. Beside the skeleton lay parts of a canopy consisting of six rods of gold and silver, four of which passed through figures of gold bulls. The richness and grave type of the Majkop royal tomb is comparable to that of Alaca Hüyük in northern central Anatolia, which itself probably belonged to an Indo-European (pre- or early Hittite) ruler. The growth of wealth coincides with the 'treasure horizon' in the Near East. By the middle of the third millennium the Kurgan chieftains had not only been well acquainted with the Near Eastern kingdoms, but also had in their possession great treasures related in general character to those known from Bronze and Iron Age royal tombs, such as Mycenaean shaft grave and tholos tombs, Únětice in central Europe (Leubingen, Helmsdorf, and others), Urnfields in central Europe (Čaka and Očkov in Slovakia, Klentnice in Moravia, Hart in Bavaria, and

others), King Midas' or Gordios' in Phrygia, and so forth. Usually royal tombs are found separately from the cemetery of the other members of the society. The graves of laborers frequently contained only a pot, a flint tool, or nothing at all. A very similar distribution of wealth indicating social inequality is traceable in later cultural groups of Kurgan origin throughout the Bronze and Iron Ages.

The burial rites indicate not only social differences, but also the man's dominant role in society: the first and central grave in a barrow usually

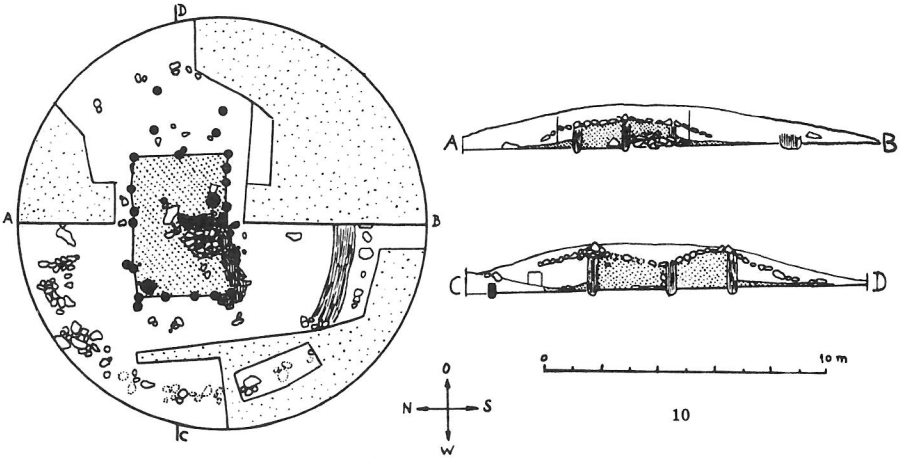

FIG. 11. Plan and cross-sections of a Late Kurgan barrow from Switzerland showing a house-like construction with two compartments. The mortuary house was surrounded with a fence—a ditch and a stone ring. Approximate date: mid third millennium B.C. *After* W. Drack.

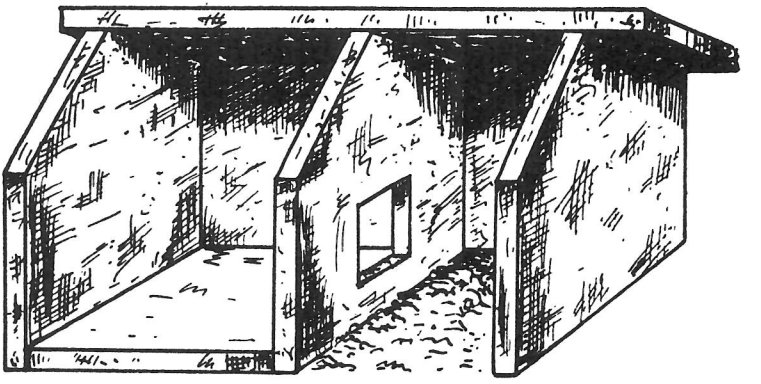

FIG. 12. The stone house from the royal tomb in barrow no. 1 at Tsarskaja (Novosvobodnaja) in the River Kuban Valley, northwestern Caucasus. The thickness of the stone slabs was from 17.6 to 26.4 cm., and their weight was from 1.5 to 2.5 tons each. *After* Veselovskii, 1898.

belongs to a man, perhaps the father of the family or the village elder, and the women's and children's graves occupy secondary positions. The old Indo-European custom that the housemaster had unrestricted right of property over his wife and children and that the wife should die with her husband is indicated archaeologically by frequent double graves of man and woman and of adult and one or two or more children buried at the same time. Parent-children relationship is shown by bone analyses.

RELIGION. From graves we know that these people had very strong beliefs about life after death, which had to continue in the same way as life on earth. Therefore graves imitated houses (Figs. 11, 12), and the dead needed their weapons, tools, pots, ornaments, and even vehicles. Animal graves and offering pits or animal bones show the importance of cults and indicated sacrifices of oxen, horses, stags, boars, dogs, bears, sheep or goats, birds, and hares. Kurgan graves north of the Black Sea usually included snake skeletons, sometimes up to ten. Human sacrifice is evidenced by the disposal of separate human bones in pits close to the grave, in some cases mixed with animal bones. Among the human bones were those of children and of adults. Skulls and leg bones of cattle and horses were found separately deposited above or beside the graves. This custom suggests that the animals were meant to be buried together with their master, but their flesh was eaten and the large bones were collected into skins and buried with the dead man—an Indo-European custom persisting into historic times. Graves of two bulls or two horses, or of a mare and a colt, a cow and a calf, a goat and a kid, a sheep and a lamb accompanying a human mother and child are known. Some of the sacrificed animals are likely to be connected with sacrifices at harvest festivals.

Kurgan house graves built of timber or of stone slabs were covered with an earthen or stone mound and then topped with a stone stela. The stela was primitively carved in a human shape and on some of them male attributes were indicated. The figure held an axe or a mace in one hand. In one case a bow was engraved. A belt and sometimes a necklace were shown (Fig. 13). The stelae may have portrayed a male divinity, probably a Thunder-god, whose figure also appeared engraved on cist-grave slabs. Hammer-axes or battle-axes made of antler, stone, semiprecious stone, or copper, that frequently occur in male graves, or engraved on rocks seem to have been more weapons of the Thunder-god imbued with God's mana than of a human warrior (Figs. 14, 15). Their symbolic character is indicated by zig-zags, rays, or other motifs used for decoration, by their appearance in a miniature size, by lack of working traces, or by the fact that they were made of nephrite, serpentine, diorite, amber, and other materials not appropriate and too expensive to be used for tools (Fig. 16). Footsteps on third millennium stelae and stone slabs very probably are connected with the beliefs in giant god's footsteps (of the Baltic *Velinas*, Slavic *Veles* type, gods of underground and fertility).

The presence of a male divinity or divinities contrasts with the preeminence of various symbolic female figurines in the Anatolian, Aegean,

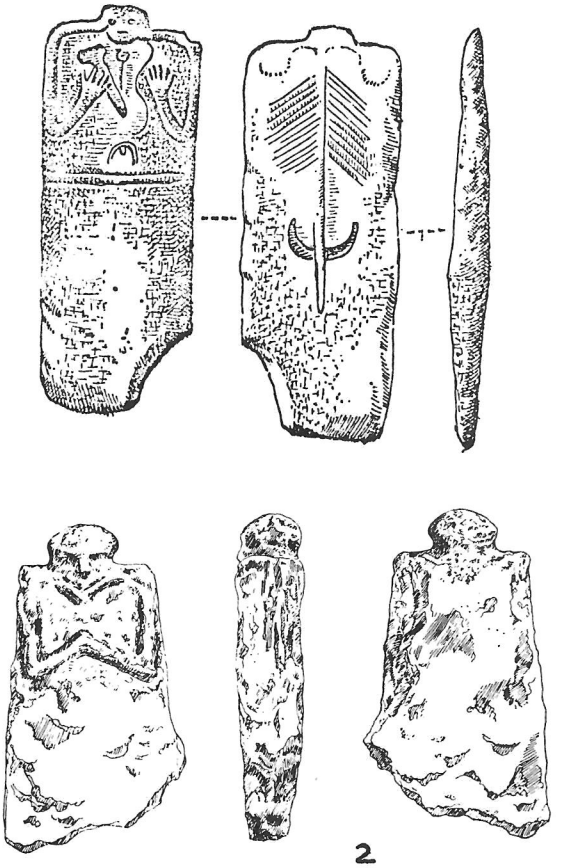

FIG. 13. Anthropomorphic stone stelae (front, back and side views) from Kurgan IV graves in the lower Dnieper area. *1*, Natalivka near Dnepropetrovsk, 1.6 m. high; *2*, Pervomaivka near Verkhnorozachinsk, district of Kherson, 52 cm. high. *After* Markovin, 1959 (*1*) and Illins'ka, Kovpanenko and Petrovs'ka, 1960 (*2*).

and Balkan cultures prior to the Kurgan expansion. However, in the early part and throughout the fourth millennium B.C., seated goddesses made of clay, marble, or alabaster appeared in the Ukraine and the northern Caucasus. These were borrowed from the people in the Balkans and the Mediterranean area. A figure of a boar in clay was found in the already-mentioned village of Dereivka (Fig. 17), suggesting that as early as the fifth millennium B.C. the boar played a role in mythology as it did throughout millennia of early history of the Indo-Europeans. Necklaces and other ornaments were made of boar's tusk. Other animals' teeth, in particular those of wolf, dog, and elk, were also used for necklaces. These occur so frequently in graves that their symbolic significance cannot be doubted. Horses' heads were carved of diorite or other stone. These heads were

FIG. 14. A limestone slab with engravings of axes, human figures, footsteps and other symbols from a stone cist grave in the cemetery of Krasnaja Gora near Simferopol, Crimea. Relative chronology: ca. 2500–2200 B.C. *After* Tallgren, 1934.

FIG. 15. Copper battle-axe ornamented with zig-zag (lightning bolt?) motifs. Isolated find from Weeze-Baal, Geldern, Western Germany.

mounted on a rod and used as scepters (fig. 2). Bull, stag and lion figurines of copper or gold are known from royal tombs of the third millennium B.C. Stag figurines were decorated with concentric circle motifs and had enormous antlers. Stags with supernatural antlers also occur engraved on rocks. They may represent winter solstice symbolism.

Among the symbols on pots, stone, bone and copper objects, we find the snake and the ubiquitous sun (Figs. 18, 19). The fact that the Kurgan people engraved the sun or wheel symbol on almost every ritually important object speaks for the prominence of the sun cult and the significance of the symbolism in connection with the rotation of the year. Carefully constructed braziers found in houses and in graves suggest the perpetuation of fire as a holy element (Fig. 20). Charcoal, ashes, or pieces of carbonized

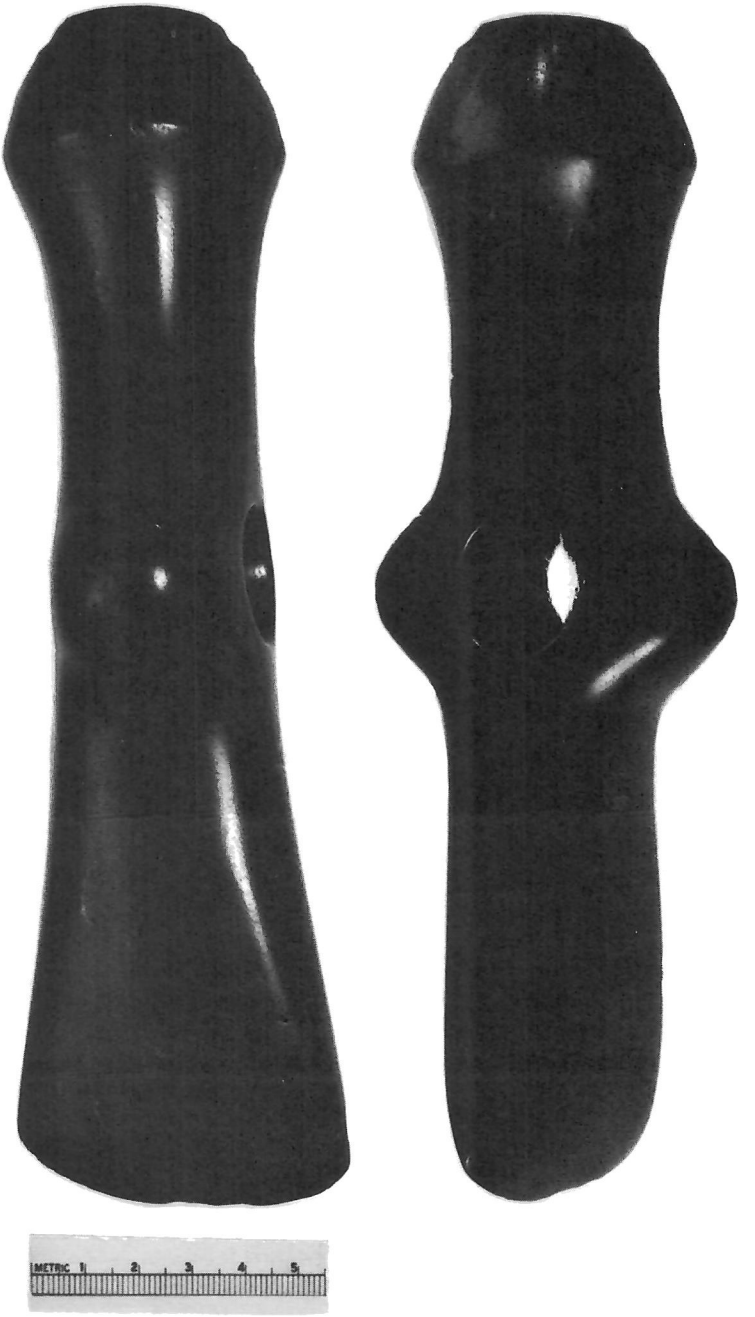

FIG. 16. Late Kurgan battle-axe of nephrite. An isolated find from central Europe UCLA Museum and Laboratories of Ethnic Arts.

FIG. 17. Small figurine of a boar made of clay, from the village of Dereivka near Kremenchug in the lower Dnieper area. The date: second half of the fifth millennium B.C. Kiev, Institute of Archaeology, excavations by D. Ja. Telegin.

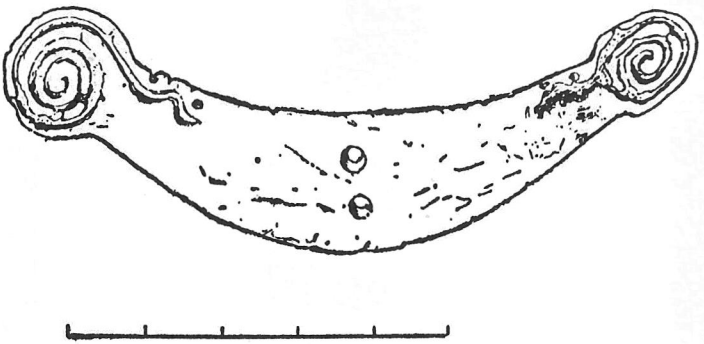

FIG. 18. Ornament made of boar's tusk decorated at the ends with incised snakes coiled in concentric circles. Found in a child's grave together with perforated animal teeth. Cemetery of Nal'chik, central Caucasus. Ca. 4000 B.C. *After* Kruglov, Piotrovskii and Podgaetskii, 1941.

cow dung in the bottom of cross-footed dishes indicate their use as braziers. The abundance of ocher, charcoal, and ashes in the graves indicates the ritual significance of fire and of the red color of ocher. Metal cauldrons and other containers may have been used in rites connected with the water cult.

III. Origins

The earliest Kurgan graves are known from the steppe area between the River Don and the Urals, north of the Caucasus range and north of the Caspian Sea. Most of them are known from the lower Volga region. West of the Don, north of the Sea of Azov, the Crimea, the lower Dnieper and

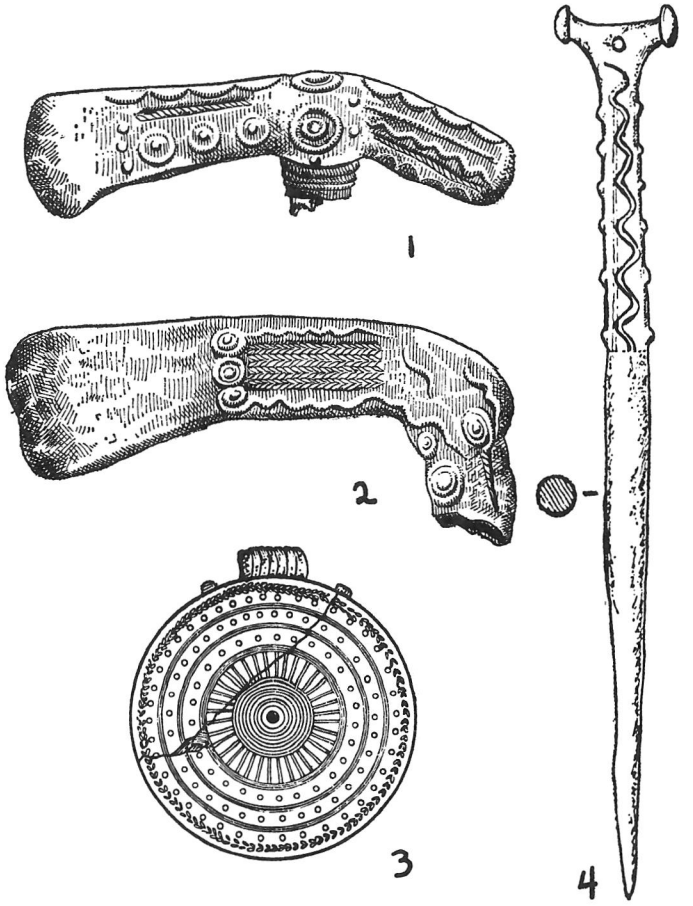

FIG. 19. Copper axes, pin, and pendant decorated with solar and snake motifs from Late Kurgan sites in the north-central Caucasus. Reproduced from Gimbutas, 1965.

Dniester region, the Kurgan people appeared as intruders. Here they found local stock-breeders and fishermen whose archaeological remains are known from such sites as Kamennaja Mogila near Melitopol, the Crimean neolithic sites and the lower Dnieper Surskaja complex.[19] Between the middle Dnieper basin and the River Donets spread the "Dnieper-Donets culture", certainly of a non-Kurgan character.[20]

Although earliest Kurgan materials are very scanty (most of the graves are without inventory, except for ocher and sheep and cattle bones; habitation sites are known from surface materials only), the present evidence points not to the North Pontic, but to the lower Volga-Kazakhstan steppes as the Kurgan homeland area. It is premature to say how far east the early

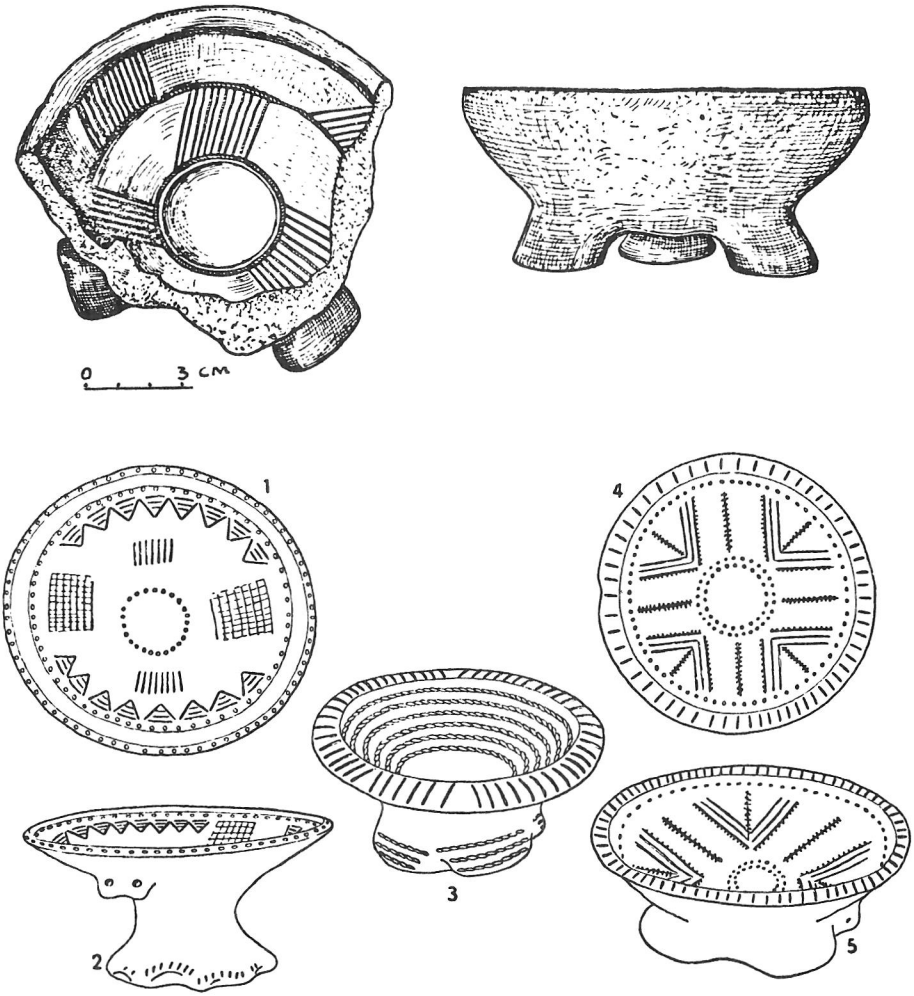

FIG. 20. *A*, Brazier—a dish standing on three legs—decorated inside with a solar pattern. From the upper stratum of Mikhajlovka. *After* Lagodovska, Shaposhnikova and Makarevich, 1962. *B*, Dishes (braziers) from the Vučedol sites in Bohemia and western Slovakia: *1–2*, Bylany, Bohemia; *3*, Iža, Slovakia; *4*, Abrahám, Slovakia. *After* E. Neustupný, 1966.

Kurgan culture extended, but at a later period it occupied almost the entire steppe belt in Siberia.

This is supported by the fact that in the Upper Yenisei area a related culture, called Afanasjevo, which belongs to the Kurgan IV period is known There are also "Pre-Afanasjevo" materials, but as yet no neolithic sites of respectable age have been excavated. Many archaeologically blank areas between the Volga and Yenisei make it difficult to speculate on the

beginnings of the Afanasjevo culture. There is, however, no doubt that the Afanasjevo and Bronze Age cultures of the Siberian steppes, the Andronovo and Tazabag'jab in the Aral Sea area, belong to the same Kurgan family and are represented by the same physical type of people.

IV. Chronology and Expansion

The above sketch of the general character of the Kurgan culture depicts the fifth, fourth, and third millennia B.C. The basic elements continued throughout the three millennia north of the Black Sea, were transmitted by expansions to Europe and the Near East, and more or less conservatively persisted in separate Indo-European groups throughout prehistoric and historic periods.

In spite of many uncertainties and a lack of carbon-14 dates for most of the chronological phases of the Kurgan culture in the steppe region until recently, archaeologists attempt classification of present materials on the basis of stratigraphy, typology, commercial relations, and a few carbon-14 dates. In my paper of 1965 (*see* footnote 1) I have given a provisional chronological scheme of the Kurgan culture during the Chalcolithic stage, dividing it into Early (Kurgan I)—fourth millennium B.C.—, Middle (Kurgan II and III)—third millennium up to ca. 2200 B.C.—, and Late (Kurgan IV), which covers the end of the third and the beginning of the second millennium B.C. I am advocating now, on the basis of the dates obtained by carbon-14 analysis, a *higher chronology* than the one I have used in my previous articles. The number of carbon-14 dates has increased, and they do not contradict the evidence obtained from stratigraphies and typologies. The current attempts to elaborate the radiocarbon dating method make dates higher. In this article I use tree-ring calibrated radiocarbon dates based on Dr. Suess' correction curves.[21]

The relative chronology of each of the periods can be established through contacts with the west, in particular with the Cucuteni-Tripolye culture.

To simplify the chronological scheme the following subdivision is given:

1. Early Kurgan or Kurgan I north of the Black Sea precedes the infiltration into (Danubian) Europe and the Near East;

2. Middle Kurgan is the period of continuous waves of expansion, subdivisible into early and late phases, the *Kurgan II* and *Kurgan III* periods.

3. Late Kurgan or Kurgan IV is the last period of a relatively uniform culture before the crystallization of Bronze Age cultures in Europe.

The Early Kurgan (Kurgan I) Period, Fifth Millennium B.C.

The beginnings of this period are not yet clear and an absolute date for the boundary between the neolithic and chalcolithic stages is impossible to establish without carbon-14 dates. From the data presently available to us, a date in the second half of the fifth millennium B.C. may be tentatively suggested. Early Kurgan finds have been shown to be contemporary with the Tripolye A and B_I (which equals the Pre-Cucuteni III and Cucuteni A

phases in Rumanian Moldavia) and Late Dnieper-Donets cultures. The mingling of finds belonging to three groups of different cultural background indicates the first encounter of the Kurgan people with the Cucuteni-Tripolye agriculturalists of the Balkan cultural bloc and with the Dnieper-Donets culture, the latter represented by massive European Crô-Magnon C type people with an admixture of Caucasoid elements, who must have settled along the lower Dnieper, the Sea of Azov, and the Donets River in an earlier period. The appearance of Kurgan materials in the lower Dnieper-Dniester basins indicates the first Kurgan advance westward.

In the terminology of Soviet scholars this is the early 'Srednij Stog II' culture. (They do not use the term 'Kurgan culture;' their 'Srednij Stog II culture' covers the Early and Middle Kurgan periods).[22] Pointed based pots were decorated with stabbing, incised lines, and shallow impressions made with a comblike stamp; there was no 'corded' pottery yet. Potsherds of this type were found in Cucuteni A_3–Tripolye B_I settlements.[23] Hăbăşeşti, a site of Cucuteni A_3 phase, yielded one carbon-14 date 3380 ± 80 B.C. A tree-ring calibrated date is ca. 4300 B.C.

The Kurgan (or 'Srednij Stog II') people inevitably took over a large territory of the Ukraine and started their expansion to the Balkans. This terminates the Early Kurgan period.

The Middle Kurgan (Kurgan II and III) Period, Fourth Millennium B.C.

This can be subdivided into two phases:

KURGAN II, during which Kurgan sites are found in the whole North Pontic area, and their traces appear west of the Black Sea in the Ukraine, Rumania, Yugoslavia, and eastern Hungary. This period is parallel to Cucuteni AB (Tripolye B_{II}).

KURGAN III is the climax of expansions and conquests in the Balkano-Danubian area, and is parallel to Cucuteni B (Tripolye CI). At this time the Kurgans appeared in the Near East.

North of the Black Sea, forested conditions prevailed. There was considerable forest fauna and freshwater fish.

KURGAN II, ca. 4000–ca. 3500 B.C. Materials from this period demonstrate continuous coexistence with the Dnieper-Donets culture: two different physical types (both of 'Crô-Magnon C' type, but with the Kurgan People being more gracile) and burial customs (collective burials in trenchlike pits characteristic of the Dnieper-Donets culture, and single burials of Kurgan type) were proved to be present even in the same villages. The Dnieper-Donets culture seems to have disintegrated during this phase. To the same phase belongs the settlement of Srednij Stog II which overlies the remains of a Dnieper-Donets village (Srednij Stog I). The existence of large cemeteries of the Mariupol' type with long trench graves containing many skeletons in an extended position, north of the Sea of Azov and in the lower Dnieper area, ended. This type was replaced by graves in stone cists or in pits under low earthen or stone barrows (in Russian called 'kamjanye zakladki') which covered single burials. The dead lay on the

back with slightly contracted legs and were equipped with flint points and arrowheads of triangular or rhomboid form, long flint knives, stone celts, battle-axes, boar's tusk necklaces, beads made of shells of perforated animal teeth (usually deer's), copper awls, bracelets, and pendants. The grave type contrasts sharply with that of the Dnieper-Donets culture, but some flint objects resemble in form and technique those of the Dnieper-Donets and Cucuteni AB (Tripolye B_{II}) cultures. Pointed bottomed pots of this phase were ornamented by stabbing, scratching, and impressions with a comblike instrument. Cord impressions were not applied yet, which shows that this technique of pottery decoration was not specifically 'Kurgan.' Its appearance was actually due to influence of the Dnieper-Donets culture.

The same type of grave appeared in the steppes west of the Black Sea, in the Danube Plain as far as northeastern Hungary, and in the northern and eastern Caucasus. In the entire area between the Black Sea and the River Tisza in eastern Hungary many barrows with shaft graves and wooden constructions of oak or beech wood, containing skeletons lying on their backs with slightly contracted legs (Fig. 21), have been discovered in the course of the last hundred years.[24] Since the graves were poorly equipped, not much attention was paid to the study of their chronology. From the grave architecture, position of the skeletons, deposition of a piece of red ocher the size of an egg, presence of horse, cattle, calf, sheep or goat, and dog bones (and in one case a bear's skull), it is seen that many of these barrows represent the Kurgan II and III types and thus are actual testimony of the intruders' presence in east central Europe.

In Transylvania this period is represented by the cemetery of Decia Mureşului in the district of Torda-Aranyos and in the northern central Caucasus by the cemetery of Nal'chik.[25] A clay (marl) figurine in the cemetery of Nal'chik has close parallels in Cucuteni AB (Tripolye B_{II}) villages. On the other hand, a copper shaft-hole axe and a stone macehead decorated with four semi-globes found in the Decia cemetery of Transylvania have close parallels in the sites of the Tiszapolgár culture of northern Hungary and Slovakia and in the latest (Mariupol') phase of the Dnieper-Donets culture. This places the Kurgan II period in the first half of the fourth millennium B.C. on the basis of its contemporaneity with the Tiszapolgár and Cucuteni AB complexes.

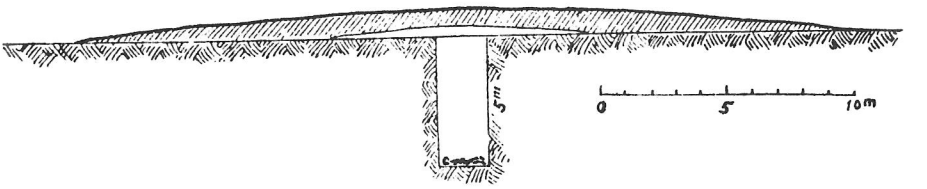

FIG. 21. Cross-section of a kurgan (no. 1) near Constanţa at the Black Sea, Rumania. *After* Schuchhardt and Traeger, 1918.

KURGAN III. Ca. 3500–3000 B.C.: Kurgan III material is well defined stratigraphically. In the lower Dnieper region, it is represented in the Mikhajlovka II layer. In Russian terminology, it is called Early 'Yamna' (Early Pit-grave culture) or late 'Srednij Stog'.

The whole Pontic area was now dominated by the Kurgan peoples alone. The number of Kurgan sites increased remarkably.

During this period the Kurgan people spread their roots in eastern central Europe and the whole Balkan area down to Macedonia by demolishing the high Chalcolithic-Copper Age cultures, and they entered Transcaucasia, Anatolia, and northern Iran.

Revolutionary changes in Bulgaria, Rumania, Yugoslavia, Hungary, Czechoslovakia, and southern Poland show that people of a different background imposed their ways of life on the autochthonous peoples. Kurgan elements become numerous during the Cucuteni B phase in Moldavia and the western Ukraine, while the Gumelniţa culture in Dobrudja, southern Rumania, and Bulgaria ended during Gumelniţa III.[26] Other cultural groups also slowly disintegrated: the *Salcuţa* in western Bulgaria and southwestern Rumania, the *Vinča* in central and southern Yugoslavia; the *Bodrogkeresztúr* in eastern Hungary, eastern Slovakia and northwestern Rumania; *Lengyel* in northern Yugoslavia, western Hungary, lower Austria, western Slovakia, Moravia, and southern Poland; *Butmir* in Bosnia; and *Hvar* in Dalmatia. These flourishing agricultural groups in east central Europe of the fifth millennium were replaced in the fourth millennium B.C. by a culture composed of elements typical of the Kurgan culture north of the Black Sea mixed with elements of the substratum culture or elements from western Anatolia. To archaeologists, this new culture is known by many names: Early Bronze Age or *Ezero* in Bulgaria and Macedonia, *Cernavoda* in eastern Rumania (Dobrudja), *Coţofeni* in Transylvania, *Gorodsk-Usatovo* in the western Ukraine, *Proto-Baden* or *Boleraz* in Hungary, northern Yugoslavia, Lower Austria, southern Germany, Czechoslovakia, and southern Poland, succeeded by the *Baden-Pecel* or *Channelled Pottery* complex. These groups are so much interrelated that they could all very well be given one name, such as 'Balkano-Danubian cultural unit'.[27] Materials of this culture are well documented in many stratified sites, for instance in the famous Bulgarian mounds of Ezero and Karanovo. The Ezero 'Bronze Age layer' of 4 meters and nine building levels indicates a long cultural existence.[28] In Rumania, two carbon-14 dates have been obtained from samples taken from fortification ditches of a settlement at Dealul Sofia near Cernavoda in Dobrudja: 2555 ± 100 and 2435 ± 100 B.C.[29] Their true age (calibrated with tree-ring chronology) is the second half of the fourth millennium B.C. The same period is indicated by the dates obtained from the Early Bronze Age levels (Period IV) of the Photolivos-Sitagroi mound in Macedonia.

In this period Kurgan elements infiltrated the upper Danube basin and gradually the northern Europe, the territory of the Funnel-necked Beaker culture during its phases C and D.

There were numerous ties with the western, central, and eastern Caucasus during the Kurgan III phase, at which time the Kurgan peoples must have crossed the Caucasus mountains and dispersed to Azerbaidzhan, eastern and northern central Anatolia, and northern Iran.

The only Kurgan grave in Azerbaidzhan known so far is that from Uch-tepe in the Mil'ska steppe near the river Karkar-Chaja, excavated in 1961 by A. A. Iessen.[30] This was a typical Kurgan grave in a large deep shaft covered with timber planks. A piece of red ocher and sherds of a number of thin-walled and undecorated pots were found in the pit. The potsherds apparently fell into the pit after a funeral feast above the grave when the mortuary house was burned.

Two carbon-14 dates have been obtained from the Kurgan grave of Uch-tepe: one from a piece of wood, 2539 ± 120 B.C., the other from a piece of charcoal, 2867 ± 230 B.C.[31] The calibrated dates indicate second half of the fourth millennium B.C.

The appearance of the Gray Ware culture in northern Iran in the period synchronous with the Early Dynastic period of Mesopotamia (ca. 3000–2370 B.C.—see Thomas' article, p. 200) and the displacement of elements of the older culture in the tells of northern Iran (Hissar II, Tureng Tepe, Shah Tepe) was probably in connection with the movements of the Kurgan peoples. The series of radiocarbon dates from the Gray Ware layer of Tureng Tepe fall into the period of first half and middle of the third millenium B.C. (information by J. Deshayes, 1969). If corrected, they belong in the second half of the fourth millennium B.C.

Late Kurgan (Kurgan IV), Third Millennium B.C.

During this period continuous waves of expansion or raids touched all of northern Europe (except the Arctic zone), the Aegean area (Greece and West Anatolia), and the East Mediterranean area possibly as far south as Egypt.

This is a period when almost all Europe possessed similar cultural elements which can be synchronized. It ended with progressively increasing cultural differentiation and the beginning of the Central European Bronze Age and the Middle Helladic period in Greece. The Late Kurgan period in the northern Caucasus is a full-blown metal age. Only north of the central European mountains did the culture remain in a chalcolithic stage.

In the lower Dnieper area it is well represented in layer III of the Mikhajlovka hill-fort. The fort was now considerably larger than Mikhajlovka II and was heavily fortified with ditches and massive stone walls. From this period date the aboveground houses having stone foundations and wattle and daub walls (Figs. 8 and 9).

The famous royal tombs of Majkop in the northwestern Caucasus and Alaca Hüyük and Horoztepe in northern central Anatolia, found in typically Kurgan grave constructions, may have belonged to Kurgan rulers. Generally they are equated with Troy II and Early Dynastic III in Mesopotamia. These fabulous grave treasures reveal a remarkable ability to

accumulate power and wealth. This suggests that the Indo-European kings were efficient rulers among the peoples who spoke other languages and who were of different cultural backgrounds (like the Hattic people in central Anatolia). We can surmise that at about the middle of the third millennium B.C. most of the lands in Caucasus, Anatolia, and Iran which in later periods are known to have been under the control of Indo-European kings had already been invaded by the Kurgan people.

In my monograph of 1956 I called this phase 'The Hut-Grave Period' because of the remains of timber huts under earthen mounds. In fact, similar hut structures are evidenced in Middle Kurgan graves also. All over the western Caucasus and in the Crimea mortuary houses were built of stone slabs. Classical examples of charnel houses, rectangular in plan consisting of two rooms and having a gabled or flat roof, are known from the royal barrow of Tsarskaja ('Novosvobodnaja' in Soviet literature) in the River Kuban valley of the northwestern Caucasus (Fig. 12).[32]

During Kurgan IV times there appeared a peculiar type of burial in pit-caves, termed 'catacombs'; in the Don and Donets river basins north of the Sea of Azov and in the lower Dnieper area. In many cases they were found as secondary burials in the earlier kurgans. Burial rites and grave contents place them in the Kurgan family, though the burial in pit-caves, consisting of a shaft and an oven-shaped pit, suggests influence from the Cyclades or Sicily. This is not surprising since the Kurgan people by now were well acquainted with many lands, including the Aegean and Mediterranean coasts and islands. There is an indisputable similarity between the North Pontic catacomb graves and those of northwestern Sicily around Palermo belonging to the Conca d'Oro culture and called there oven-shaped tombs, with shaft and chamber presumed to be of Aegeo-Anatolian origin.[33] Not only is the grave shape in these two areas closely related, but also the manner of burial and the type of grave goods. Among these are red ocher, bone tools, small copper daggers, rings and bracelets, and pots decorated with ornamental motifs (cord impression, lines of incisions and dots) typical of the Kurgan IV period. The cave site of M. Cronio near Agrigento in southern Sicily had a layer containing pottery decorated in Kurgan IV fashion covering Diana and Serra d'Alto deposits.[34] Around Taranto, Brindisi, and Foggia in Apulia a number of sites yielded pottery, flint, and bone tools of Kurgan IV character.[35] Catacomb-graves excavated in 1966 in Laterza near Taranto are nearly identical in shape with those north of the Black Sea.[36] Grave goods included flint and bone arrowheads, stone arrow straighteners, perforated animal teeth, and other artifacts closely related to those in Pontic catacomb-graves. Decorative motifs on bowls, dishes, cups, and amphorae have a striking resemblance to those on catacomb-grave ceramics in the Pontic region and Kurgan IV pottery in the Vistula and Oder basins.

Marble, nephrite, serpentine, and other semiprecious stones were used by Catacomb-grave people for their mace-heads and battle-axes. Silver was used for earrings or hair-rings, and greenish or white faience, carnelian,

and mother-of-pearl for beads. All these materials indicate wide commercial contacts. Traces of local metallurgy were present all over the North Pontic area and metal types were highly influenced by the Kuban center of metallurgy during its 'Middle Kuban' stage. Solid wheels, whole vehicles (Fig. 22) and miniature models of two-wheeled carts are quite common finds in Kurgan IV graves; they could have served as hearses or were inhumed as symbols of a vehicle to the other world.

Some scholars think that the introduction of burials in pit-caves north of the Black Sea is an indication of the appearance of new people.[37] During the Catacomb-grave phase there appeared such customs as artificial deformation and trephination of the skulls of upper class people. These practices were known in later periods to the people of Kurgan derivation but are so far not recorded from the earlier Kurgan periods. During this period burial customs, except for the grave structure itself and the usage of barrows, remained the same as customs used in mortuary houses of timber and stone. Without the application of quantitative analysis and thorough studies of physical anthropological data, the problem of immigration of new people cannot be solved. The author sees as a possibility a reflux movement of the Kurgan people, not an incursion of non-Kurgan peoples.

The Kurgan IV movement proved to be connected with the Vučedol complex in the Middle Danube basin (northern and Adriatic Yugoslavia, Hungary, western Slovakia, and lower Austria), in which surprisingly similar cultural elements are found: hill-forts, catacomb-graves, symbols, richly decorated braziers, etc. The Vučedol culture succeeds Baden and precedes the Bell Beaker complexes. During its classical phase, represented in the hill-fort of Vučedol,[38] indubitable contacts with the eastern Mediter-

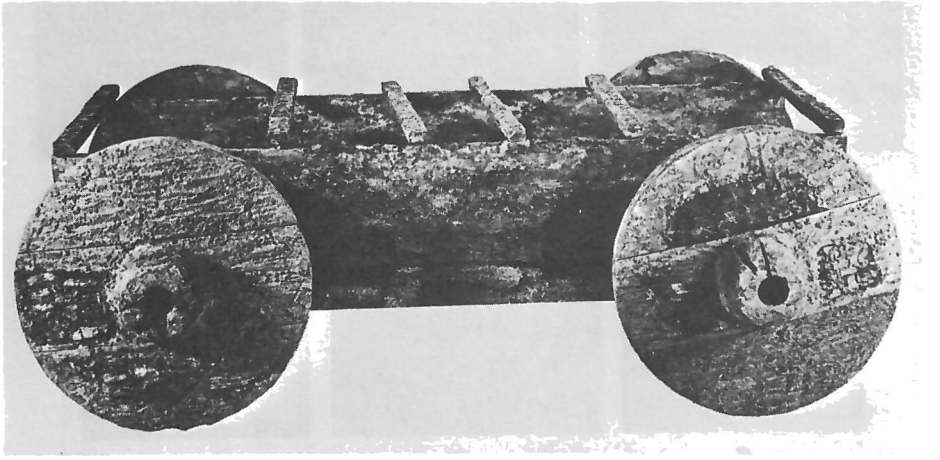

FIG. 22. Four-wheeled wagon from a catacomb-grave at Kudinov, lower Don. Relative date: 2300–2200 B.C. Excavations by Melent'ev, Archaeological Institute, Leningrad.

ranean world, particularly with the final phase of Early Minoan and beginning of Middle Minoan Crete, are shown by exotic finds such as horns of consecration, altars, dove sculptures, engraving of double-axe symbols (Fig. 23), and the clay model of a four-wheeled wagon at Palaikastro in eastern Crete, decorated in much the same way as one altar in the Vučedol site (Fig. 24). We may surmise that it was not difficult for the Vučedol people to use the Adriatic and East Mediterranean waterways. Kurgan IV corded and Vučedol type white incrusted pottery was discovered in a stratified cave site at Gudnia, 55 m. north of Dubrovnik on the Adriatic coast of Yugoslavia.[39] This site, excavated in 1964–66, is another proof of the presence of Kurgan IV people in the Adriatic area and, indirectly, of their navigation in this sea.

The Bell Beaker complex, an offshoot of the Vučedol bloc (more precisely, of the Zók-Makó group in Hungary) continued Kurgan characteristics. The Bell Beaker people of the second half of the third millennium B.C. were vagabondic horse riders and archers in much the same way as their uncles and cousins, the Corded people of northern Europe and Catacomb-grave people of the North Pontic region. Their spread over central and western Europe to the British Isles and Spain as well as the Mediterranean islands terminates the period of expansion and destruction.

Materials from barrows in the northwestern Caucasus, such as from the princely burials at Tsarskaja, Ul', and a series of rich grave inventories in

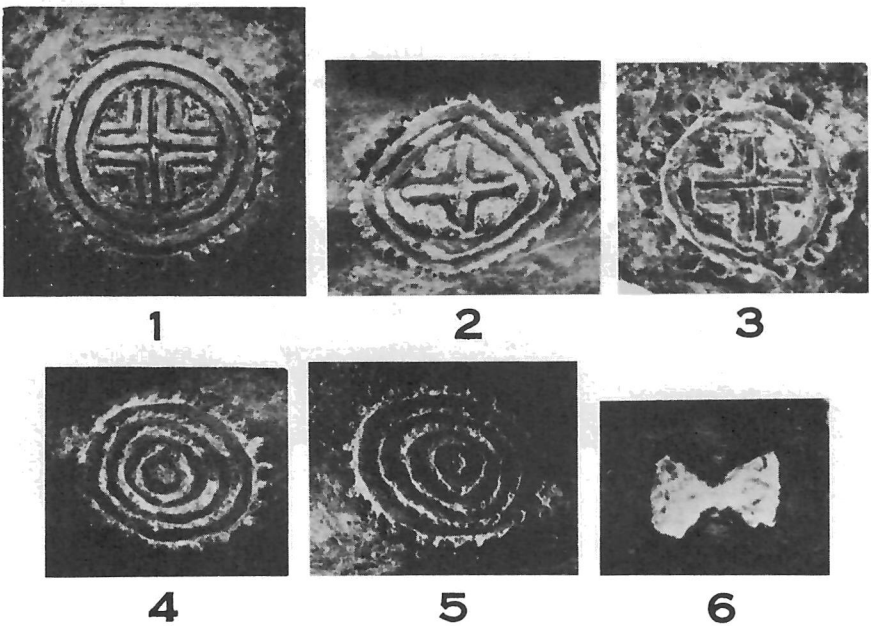

FIG. 23. Solar and double-axe (bottom, *right*) motifs incised and white incrusted on pots from the Vučedol period, Vučedol hill-fort. *After* Schmidt, 1945.

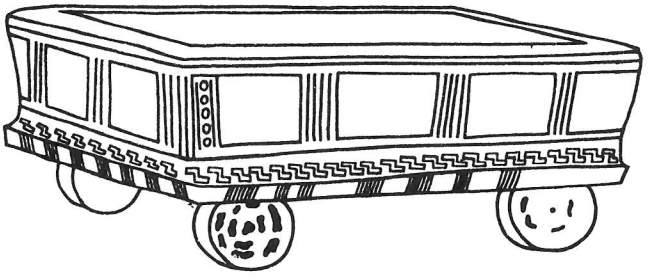

FIG. 24. Model clay wagon (with polychrome decoration) from Palaikastro, a Middle Minoan Ia site in eastern Crete. *After* Evans, 1935.

the upper Terek region, indicate contemporaneity with the Akkadian, Post-Akkadian, and Ur III times in Mesopotamia, Carchemish graves, Hama J4 and Amuq J in Syria, and with Hissar III C in northern Iran. This covers the period from 2400/2300 to the beginning of the second millennium B.C. In the northern Caucasus this is the so-called 'Middle Kuban period of metallurgy' following the period of Majkop and beginning with rich inventories of the Tsarskaja (Novosvobodnaja) royal tombs: tanged daggers, spearheads, shaft-hole axes, copper cauldrons, ladles, bidents, tridents, and human figures, gold and silver pins, rings and pendants. Assemblages following the Tsarskaja phase are typified by hammer-headed pins. Hammer-headed pins were made of copper and were imitated in bone in the areas north of the center of metallurgy in the Caucasus and in central and northern Europe.

The first carbon-14 dates for catacomb-graves were obtained from the University of California at Los Angeles laboratory. The wood sample came from a catacomb-grave in a barrow at Kudinov, county of Bagaevo in the lower Don valley.[40] The tree-ring calibrated radiocarbon age was shown to fall within ca. 4200 and 4400 B.P. or ca. 2250–2450 B.C.[41] The sample was taken from a grave which included a tanged copper dagger (knife or razor) with a widening blade (as reproduced in Fig. 5) and a footed dish (similar to that reproduced in Fig. 20*A*).

Kurgan elements appeared in the territories of the northwestern European Funnel-necked Beaker culture, East Baltic Comb-marked Pottery culture and Central Russian Pit-marked Pottery culture. The 'Corded Pottery or Battle-Axe Culture,' 'Single-Grave' (in Denmark) or 'Protruding Foot Beaker' (in Holland) culture continued to the beginning of the second millennium B.C. There is a series of carbon-14 dates in the Netherlands and Germany obtained from samples of the corded pottery graves and settlements; they cluster around 2400–2200 B.C.[42] in radiocarbon years, which corrects to approximately 3000 B.C.

The so-called "Pan-European Phase" of the Corded Pottery-Battle-Axe Culture has now been proved to be not later than the middle of the third millennium B.C. as indicated by the tree-ring and corrected carbon-14

dates obtained from the settlement at Auvernier near Geneva and regraded to be the most reliable dates so far. The "Auvernier layer" which contained corded sherds of the "Pan-European Corded-Battle-Axe Phase" gave the date of 2450 B.C. (Suess and Strahm, *Antiquity*, June 1970).

Corded pottery sherds, mace-heads, battle-axes and necklaces made of wild animal teeth, which appear in Early Helladic II-III sites such as Eutresis and Hagia Marina,[43] or Hágios Mámas and Kritsaná in Macedonia,[44] are virtually identical to those known from Kurgan IV sites north of the Black Sea (for instance from Mikhajlovka III) and also to those from Kurgan IV sites in central and Baltic Europe.

Around 2300 B.C. destruction is traceable in Anatolia and in the Aegean area. This marks the end of an era in the Aegean world. During this phase in Greece the Early Helladic II settlements of Lerna,[45] Tiryns, and Asine were destroyed, as were Troy II, Polatli II, Beycesultan XIII, Tarsus, and others in Anatolia. Widespread destruction is followed by an overall decline in material culture and evidence of settled occupation becomes rare. This destruction in Western Anatolia is possibly connected with the invasion of Luwian-speakers.[46]

The next territory where changes in cultural developments may have been caused by invasions of the Kurgan people was Syro-Palestine. The Palestinian archaeologists unanimously agree that at the end of the Early Bronze Age in Palestine there is a complete and absolute break in which the town-dwellers were succeeded by seminomadic pastoralists who had no interest in walled towns. According to Dr. Kenyon, from at least the twenty-fourth to the twentieth centuries B.C. Syria and Palestine were overrun by nomads, among whom the Amorites predominated.[47] Since the destruction of cities was completed during the course of the twenty-third century, and as there is no evidence on Amorites which takes us back past the twenty-first century B.C., the actual destroyers of the Syro-Palestinian towns very probably were not Amorites but Proto-Indo-Europeans. To the end of the Early Bronze Age and the Intermediate Bronze Age period belong barrows of Kurgan appearance known from the Levant as far south as the Negev desert in Israel.[48] Research on these barrows has only recently begun, but there is already good evidence that most of the cultural elements and finds are Kurgan. Some evidence comes from Bâb edh-Dhrâ in Jordan,[49] a walled town on the eastern shore of the Dead Sea, which was destroyed about 2300 B.C. or slightly later. Near the city was a cemetery which included graves of several periods. Tombs contemporary with the Early Bronze Age town were of charnel house type. These occasionally contained copper daggers and battle-axes, which may suggest the presence of the Kurgan people in the area of the Dead Sea before the destruction of towns. The later graves, dating from the post-destruction period, were of shallow cist type covered by a stone barrow containing single burials in a contracted position, unpainted pottery, copper daggers, and stone axes. These contrast sharply with the multiple burial tradition and the red slipped and burnished pottery of the previous phases.

The graves of Bâb edh-Dhrâ can hardly be linked with the Amorites, nor can many other tombs from this period which we shall mention below. In fact, archaeological remains of the Amorites are not known; their existence is attested only by written records.[50] The tradition of building houselike stone cists covered by barrows is known from the northern Caucasus. Close parallels are house graves built of stone slabs at Tsarskaja in the River Kuban basin, which succeeded the Majkop royal barrow and should date from around the twenty-third or twenty-second centuries B.C. Shaft-hole axes and tanged spearheads found in Tsarskaja and related sites in the northern Caucasus are known from the north Syrian Karchemish graves, the tell of Til Barsip, and from level J4 of the Hama tell which is the first level after destruction. This confirms the above-mentioned date for Tsarskaja. Many other western and northeastern Caucasian as well as Crimean stone cists are also similar. In addition to the Kurgan dispersal along the eastern Mediterranean coasts, there were movements of the Caucasian, non-Kurgan peoples represented by the diffusion in Syro-Palestine of the Khirbet-Kerak complex, which is a part of the east Anatolian-Transcaucasian Early Bronze Age culture.[51] Their migrations apparently were connected with the disturbances caused by the Kurgan people. Not without connection are Mesopotamian legends ascribing the weakening of the Akkadian empire to some strange northern tribes, which enabled the Gutians to destroy it entirely and lay the land bare for about one hundred years during the 23rd and 22nd centuries B.C.

Other colonists in Palestine not unrelated to the Kurgan people are known from Jericho, where Kathleen Kenyon has excavated 177 catacomb-graves (or pit-caves) at the famous tell.[52] Similar tombs are also known at Ajjul, Duweir, and Megiddo tells. As yet no one has thought to link these graves with the Kurgan culture, but the people buried in them are regarded by Dr. Kenyon to have been pastoralists rather than agriculturalists and definitely not town dwellers; they lived on the sites of earlier towns but contributed nothing to their development. To this author the catacomb-graves of Jericho are surprisingly similar to those known from the area between the Dniester and the Kuban rivers, north of the Black Sea and to those sporadically known from Sicily and northwestern Yugoslavia. The excavator has subdivided them into square-shaft type, dagger type, pottery type, bead type (or poor graves) and outsize type—a subdivision which would also be applicable for the Pontic graves. It indicates social stratification, sex and age differences, and special burial rites involving animal sacrifices and other customs (the latter connected mainly with outsize type graves). To the last detail many of the Jericho graves repeat the elements of the Pontic graves: the size and the shape of the grave (Fig. 25), the position of the dead, and the contents and placement of grave equipment. The dagger was the most usual metal weapon in Jericho as it was in male graves of the Kurgan culture in Europe (if not a copper dagger or knife then there was a flint dagger or knife placed near the hand). In warriors' graves in Jericho copper or bronze fittings were found, some

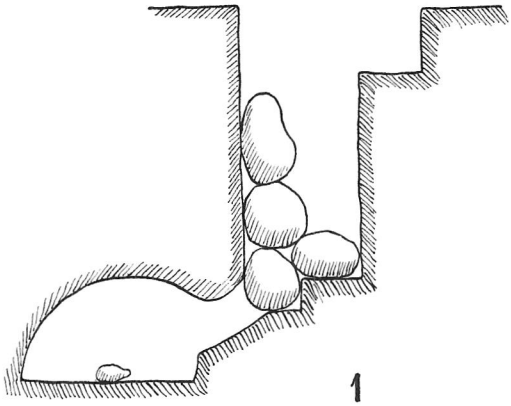

FIG. 25. Catacomb (or pit-cave) graves from *1*, the Black Sea area, and *2*, Jericho, Jordan. *After* Gorodtsov, 1907, and Kenyon, 1965.

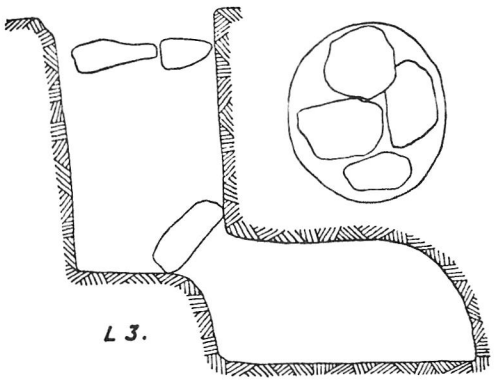

FIG. 25 Part 2

of which were shown to have belonged to wooden shields. The shield and dagger (or sword in the later Bronze Age) remained characteristic of warfare throughout the many centuries of prehistory and history of the Indo-European tribes.

In rich male graves at Jericho appeared copper head bands, pins, and bead necklaces of carnelian, crystal or blue, white or pinkish paste as in Kurgan upper class or chieftain's graves. Fragments of linen, wood, baskets, skin, and leather (in one case a leather purse) indicate the existence of additional grave equipment with perishable materials, a fact well evidenced in the graves of Pontic pastoralists. Brown powder found among earth may indicate traces of food offerings. Sheep or goat bones were quite usual in Jericho graves as they were in catacomb-graves, placed as symbolic offerings for the dead after the funeral feasts. A sample of bread from a

tomb in Jericho was of a substance believed to be millet. Millet grains, as has already been mentioned, were found in Kurgan IV graves north of the Black Sea; two pots were frequently placed at the entrance to the catacomb, one filled with millet grains, the other probably with burned meat as indicated by the remaining dark-brown substance. Bulging vessels from Jericho graves usually with flat bases, with or without handles were certainly not imported, but their decoration with a row of incisions around the shoulders links them with the simple Kurgan style. Daggers, javelins, and probably all other metal objects obviously were not imported from distant lands, and definitely not from the Ukraine; their shapes were typical of the Anatolian-east Mediterranean traditions during the last centuries of the third millennium B.C.

A number of Jericho skulls were trephined. The round trephine holes are the same as commonly found in cases from catacomb-graves north of the Black Sea and from Bell Beaker graves in Europe. The examination of skeletal materials from Jericho has indicated that these people were distinctly different from the early Mediterranean and Near Eastern populations. They were tall-statured (mean estimated stature for men 170.32 cm), with skulls within the limit set for dolichocephaly. The head was moderately high in relation to its length, and high in relation to its breadth. Their facial characteristics were mesoprosopic with mesorhine noses and chamaeconchic orbits. Although studies of skeletal materials of Kurgan people in Europe and the Near East are not yet sufficient, a general relationship can be seen between the Jericho population buried in pit-caves and the catacomb-grave people north of the Black Sea. This is the Crô-Magnon C type.[53]

The above-mentioned period of pit-cave burials at the Jericho mound is labeled by Dr. Kenyon as Intermediate Early Bronze-Middle Bronze Period. It corresponds to the First Intermediate of Egypt which is associated with the movements of Asiatic peoples responsible for the breakup of the Old Empire of Egypt. Perhaps it would not be irrelevant to mention here the fact that among the portrayals of Asiatics on Egyptian monuments dating from before the twentieth century B.C., there is evidence of Asiatics without Semitic features.[54]

If the invaders in Palestine can be identified with the Pontic Late Kurgan catacomb-grave people, then the Kurgan IV peoples can be regarded as Peoples of the Sea who raided and settled East Mediterranean islands (Sicily, Crete, Cyprus) and coasts (Apulia, Greece, western Yugoslavia, Western Anatolia, Palestine, and Egypt) just as their successors did one thousand years later, around 1200 B.C.

With the Late Kurgan migrations ended the long cycle of movements which had continued for nearly 2500 years and through which the common Indo-European language was disseminated from a limited area to the whole continent of Europe and many parts of the Near East. During the period of wanderings their language must have gone through a process of increased differentiation and during Late Kurgan times, we may guess, there existed many dialects and even languages. Europe and parts of the Near East

became culturally transformed. The next stage was the formation of separate individual cultural units, many of which continued Kurgan characteristics up to historic times and continue to the present period.

The reasons for the Kurgan explosion were obviously based on economics and social structure, and throughout the many centuries their continuous need for expansion fermented the psyche of the vagabonds. Herodotus characterized this psyche in his description of the Thracians, to whom to live by war and plunder is of all things the most glorious.

Conclusions

In reference to Proto-Indo-European culture, the archaeological evidence shows the following:

1. The Kurgan culture in the Pontic and Volga steppes of the fifth and fourth millennia B.C. was fairly homogeneous. Therefore it can be regarded as a proto-culture of all later Kurgan manifestations in Bronze and Iron Age Europe and the Near East.

2. The process of its formation is not yet clearly demonstrated by archaeological data. The present evidence shows three or four mesolithic and neolithic culture groups north of the Black Sea. The Kurgan culture most probably developed from a Mesolithic group in a limited geographical area between the Don River and the southern Urals.

3. During the fifth millennium B.C. the Kurgan people were in possession of domesticated horses and the vehicle was probably introduced to the steppe area in the fourth millennium B.C.

4. Kurgan elements in economy, habitation pattern, social structure, and religion fully agree with the common words in Indo-European languages which have been used by linguists for the hypothetical reconstruction of the mother culture.

A. Ecology and Economy. Floral and faunal remains from the early Kurgan period, which extends from the middle of the fifth millennium to the start of the fourth, show a splendid coincidence. In the list of trees there are the following: oak, birch, fir, elder, elm, ash, aspen, willow, and beech. In the list of wild animals there are aurochs, elk, boar, wild horse, bear, wolf, fox, beaver, squirrel, and badger. There were also snakes and frogs. All these remains indicate forested conditions and a damper and warmer climate in the Pontic steppe area than at the present time. All domesticated animals—sheep, ox, steer, cow or cattle, pig and horse—are evidenced. Cattle and horse-raising was the principal economic activity. This agrees very well with the linguistic reconstruction: agriculture coupled with stock breeding. Grain or 'seed' [**sēmn̥*] is attested. The wooden ard [from the root **ar(ā)*] was probably used universally. They reaped [**kerpō*] their harvests. Wool and flax were known.

B. Technology. The Kurgan people were using objects made of pure or arsenical copper from the fourth millennium onward. (A copper and tin alloy was not known to them, hence they were not acquainted with bronze.) It is not surprising that there is no one Proto-Indo-European word for

'metallurgy'. Metal could have been borrowed first from the Balkano-Danubian area via the Cucuteni-Tripolye and the Dnieper-Donets cultures, and, in the third millennium B.C., from the Near East. [**roudhos* could well be derived from the Sumerian *urud(u)*]. The usage of vehicles well agrees with the existence of the root **veĝh* in many Indo-European languages, as well as with words for wheel, axle, yoke, pole, lynch-pin, and nave. Common words for boat, rudder, and oar indicate the usage of boats. The latter are not proven archaeologically because wooden objects require special conditions for preservation, but it can be surmised that the Kurgan people used their rivers for navigation and that from the beginning of their expansion they navigated in the Black Sea and also in the Aegean, Mediterranean, and Adriatic seas. Both language and archaeology show that the Kurgan people were fairly mobile.

C. Social Structure. The presence of hill-forts [Greek *pólis*, Lithuanian *pilìs*, Sanskrit *pur-as*] with royal houses or palaces and living quarters for the ruling class, and of small villages in the vicinity of the tribal center, conforms with the linguistic evidence which indicates that early Indo-European society was divided into castes, the warrior nobility and laboring. The stratified society is proved by the earliest Greek, Hittite, Indo-Iranian, and Roman sources.

Studies in Proto-Indo-European kinship have shown that Proto-Indo-European culture had patriarchal, patrilocal families that probably lived in small houses or huts. Villages were small and presumably exogamous. Both internal and external evidence for patriliny is excellent. The whole system seems to be typical of the patrilineate in its most highly developed form.[55] Archaeological data does not contradict this reconstruction; on the contrary, it supports it.

D. Religion. Elements in the religion of the Kurgan people suggest the existence of a thunder-god, sun, fire, horse, bull, wolf, dog, boar and snake cults, very strong belief in life after death, frequently practiced animal and human sacrifices, and not highly developed ceremonialism. No real sanctuaries have ever been found; they probably had open sanctuaries. Although the archaeological reconstruction of religion cannot be complete, the data available splendidly agree with the ancient elements of Indo-European mythology reconstructed on the basis of comparative mythology and linguistics.

5. The increasing herds and population, the appearance of vehicles and fighting carts, and the use of horses for riding obviously were decisive factors for the expansion of the Kurgan warrior nobility. The following stages of expansion can be recognized:

a. Expansion to the Balkano-Danubian area in the first half of the fourth millennium B.C. (Fig. 27) and subsequently to northern Europe.

b. Expansion to Transcaucasia, Iran, and parts of Anatolia during the second half of the fourth millennium B.C.

c. Raids and/or expansion to the Aegean and Adriatic area and to Syro-Palestine and possibly Egypt around 2500–2200 B.C.

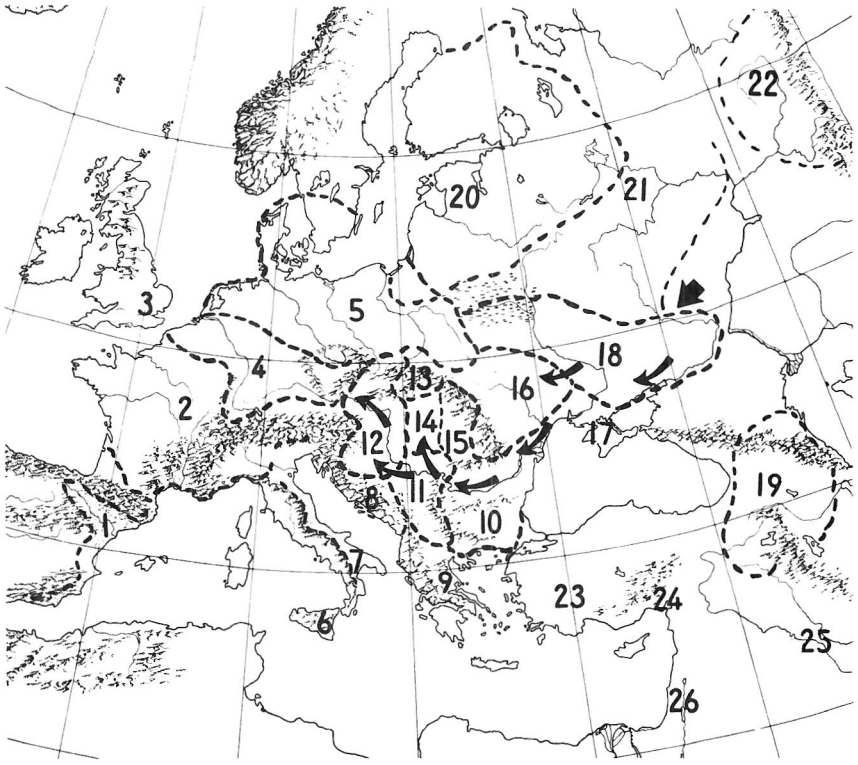

FIG. 26. European and Near Eastern cultures during the fifth and fourth millennium B.C., and the Kurgan infiltration (arrows) into the Danube basin in the first half of the fourth millennium B.C.

Key; 1. Western Neolithic groups in the Iberian Peninsula (Fossa graves, Almeria, Alicante); 2. Western Neolithic Chassey (France)—Cortaillod (Switzerland)—Lagozza (Liguria) complex; 3. Western Neolithic Windmill Hill; 4. Michelsberg; 5. Funnel-necked Beaker (TRBK) complex and its relatives in central Germany and western Czechoslovakia; 6–8. Circum-Adriatic Neolithic culture: *6*, Sicilian, *7*, Southeast Italian, and *8*, Danilo-Hvar-Butmir groups; 9. Late Neolithic (or Chalcolithic) of Greece (including Dimini, Rakhmani, Larisa in Thessaly); 10. Gumelniţa; 11. Vinča; 12. Lengyel; 13. Bükk; 14. Tisza and Tiszapolgár-Bodrogkeresztúr; 15. Petreşti; 16. Cucuteni-Tripolye; 17. North Pontic; 18. Dnieper-Donets; 19. Caucasian (also called "Kuro-Araks," "Eneolithic," "East Anatolian Early Bronze Age," and "Kirbet-Kherak."); 20. East Baltic Comb-marked Pottery culture (or "Narva"); 21. Central Russian Pit-marked Pottery culture; 22. East Russian—Middle Ural; 23. West Anatolian Chalcolithic (Beycesultan); 24. Cilician, Tarsus and North Syrian Late Chalcolithic (Mersin XII–XVI; Amuq E,F); 25. Mesopotamian culture during the Ubaid and Uruk periods; 26. Chalcolithic in Syro-Palestine (Ghassulian, Late Chalcolithic Jericho).

Note: The word "expansion" should be understood as an "infiltration" performed by Indo-European warriors who subsequently formed a superstratum in conquered lands.

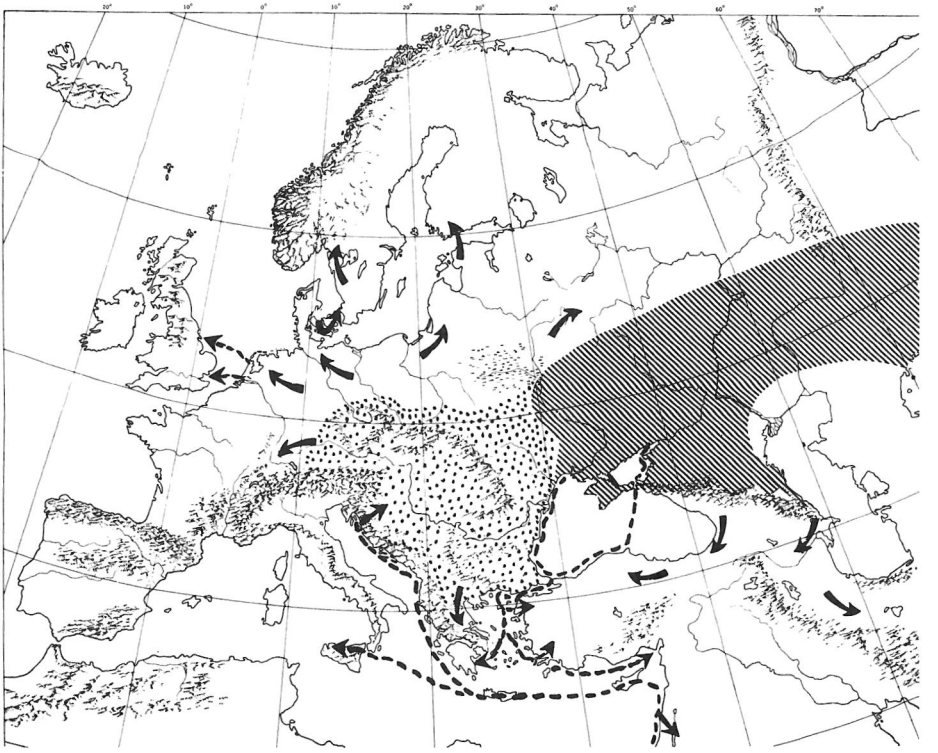

FIG. 27. Kurgan culture during the fourth and third millennia and the climax of expansions. *Solid gray area:* Kurgan culture in the Eurasian steppes. *Dotted area:* The area infiltrated not later than 4000–3500 B.C. *Arrows* show tentative movements after ca. 2500 B.C. *Disconnected line and arrows* indicate possible sea-ways, raids, and destruction at ca. 2300 B.C.

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a hill. The excavations at Tel Yeroham are reported in *Yediot* XXVII (1963). Moshe Kochavi has completed a dissertation on this intermediate period or the beginnings on the Middle Bronze Age in southern Israel: *The Settlement of the Negev in the Middle Bronze* (*Canaanite*) *I Age*, submitted to the Senate of the Hebrew University on April 1967. This study gives detailed information on the question.

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New Evidence for Dating the Indo-European Dispersal in Europe

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In 1958, the Natuurkundig Laboratorium der Rijks-Universiteit Groningen published a radiocarbon date of 2245 ± 120 B.C., which is here calculated at 2370 B.C. on the basis of the new half-life for carbon-14. This measurement was for a charcoal sample from the central grave of tumulus I at Ede in Gelderland. The grave contained an early beaker with protruding foot, decorated with cord impressions, a faceted battle-axe and a flint blade.[1] This date seemed high, ridiculously high, in terms of our traditional chronologies which had placed the earliest corded ware no earlier than the beginning of the second millennium B.C.[2] Pittioni's attribution of the Corded Ware culture to the middle of the third millennium was rejected by most British and American prehistorians as impossible from the point of view of any chronology.[3] In 1963 an even higher chronology was indicated when the Groningen laboratory published a date of 2470 ± 55 B.C. (old half-life) or 2602 ± 55 B.C. (new half-life) for charcoal from a flat grave with a similar protruding-foot beaker found at Anlo in Drenthe.[4] These Dutch dates for the Corded Ware cultures, which have long been associated with the Indo-Europeans, were in direct conflict with the traditional dates of these cultures which had been fixed chronologically to the period of the Minyan pottery in Greece, assigned to Middle Helladic I, the Hittite invasion of Anatolia, dated to the Middle Bronze Age, and the Hyksos invasion of Egypt.

The two isolated Dutch dates would have meant little and would have been easily dismissed if other discoveries had not been forthcoming in the late 1950's and early 1960's. In 1957 the excavations at Lerna, in Greece, by the University of Cincinnati and the American School of Classical Studies at Athens uncovered the 'House of the Tiles' (Lerna III) in excellent Early Helladic II context.[5] The discovery of this palace and its associated fortifications has revolutionized our view of the character of Early Helladic II civilization, which was regarded until recently as a period of small towns still very much dependent upon agriculture.[6] Even more important was the discovery that Lerna III ended in catastrophic destruction. This destruction took place at the same time as that of the great *tholos* of Tiryns, the tiled buildings of Asine and the houses at Zygouries.[7] This was

also the period of the destruction of the Anatolian sites of Troy II, Beycesultan (Early Bronze II), Polatli II and Tarsus (Early Bronze II).[8] Today it is considered by many that this widespread destruction horizon, which extends from Greece across Anatolia to Cilicia, must be directly or indirectly attributed to the invasions of the Indo-Europeans.

Proof for such an attribution includes the gray pottery with new shapes (including the two-handled bowl) which was found in Lerna IV (Early Helladic III).[9] This is Minyan pottery, which continues into Middle Helladic times and marks a culture which evolved into the Mycenaean civilization, for which Linear B tablets offer the linguist and historian their necessary historical guarantee. The horse, which the philologist has long associated with the Indo-Europeans, offers further evidence that these invaders were Indo-Europeans. Bones, which are very likely those of horse, have been reported from finds datable to the Early Helladic III horizon at Troy IV.[10] There are also the horse bones which were found in Early Cypriote III context in tomb 322B at Lapithos on Cyprus.[11] More important is W. B. Emery's report of the discovery in 1959 of a horse burial at the fortress of Buhen in Nubia.[12] It was found 'lying directly on the brick pavement of the Middle Kingdom rampart' covered by a deposit 1.15 meters in depth. Emery's work clearly indicates that the horse had reached Egypt during the Middle Kingdom, 200 years before the coming of the Hyksos.

Although the appearance of the horse in Egypt and on the island of Cyprus does not mean that the Indo-Europeans themselves reached these areas, nevertheless the Buhen discovery suggests that they must have entered southwestern Asia during the third millennium, much earlier than was formerly thought. Until recently, most historians and archaeologists associated the coming of the horse with the Hyksos, an event to be placed firmly in the second quarter of the second millennium B.C. There was excellent archaeological evidence with which to support the assumption that the Indo-Europeans and the horse came to the Near East in the early second millennium B.C.

The dating of the arrival of the Indo-Europeans in southwestern Asia to the third rather than the second millennium is perhaps supported by the recent reevaluation of the Gray Ware culture of northern Iran. The recent chronological study of R. H. Dyson suggests that the Gray Ware culture, which is known from sites such as Tureng Tepe, Shah Tepe, and Hissar II, must date somewhat later than previously assumed, i.e. to the Early Dynastic rather than the Uruk-Jamdat Nasr period as proposed by McCown, who tied the Hissar gray ware to the Uruk period.[13] Reevaluation of Mesopotamian connections by Dyson suggests that Hissar IIB should be equated with Early Dynastic II–III. The significance of the penetration of the Gray Ware culture into northern Iran is not fully understood and probably will not be until there is further excavation in the Trans-Caucasus and southern Turkestan. One can only speculate that the appearance of gray ware alongside older Hissar I wares in Hissar IIA and then the displacement of all older elements in Hissar IIB suggest first a gradual pene-

tration and then a taking over of northern Iran by a new people. On the basis of current Mesopotamian chronology, this would have taken place between 3000 and 2370 B.C.[14] The Hissar III culture, which developed from the Hissar II, lasted down to 2000/1900 B.C., after which the sites of northern Iran are deserted, suggesting that the region had passed into the hands of nomadic peoples. On these grounds, Indo-Europeans—perhaps Indo-Iranians—would be associated either with the Gray Ware culture or the succeeding nomadic phase. The coming of the Indo-Europeans to northern Iran, which is more likely to be equated with the penetration of the Gray Ware culture, could have introduced the horse into southwestern Asia. One is still left with the puzzling question of the origin of this gray ware and its relationship, if any, to the Minyan gray ware.

Archaeological work in Anatolia suggests that the movement of peoples and consequent cultural changes were much more complex than those which we can as yet only surmise for northern Iran. Troy itself gives evidence of two, and perhaps three, major destructions marking cultural changes that took place long before the time of Homeric Troy. The Troy I culture, which evolved from the late Chalcolithic within northwestern Anatolia, was destroyed ca. 2600 B.C. by a movement of peoples who pushed the Troy I peoples westward into Macedonia, where they established the Early Bronze culture, and southward into Anatolia, where they destroyed Beycesultan and account for the Troadic elements found in subsequent levels.[15] Around 2300 B.C. Troy II was destroyed by a new movement of peoples. This destruction took place at the same time as that of Polatli and Tarsus and of towns in Greece at the end of Early Helladic II. This wave of destruction was followed by the rise of an Early Bronze III culture in southwestern and southern Anatolia which continued without interruption until the end of the Bronze Age.[16] Since this was the region of the Luwians as early as the time of the Hittite Old Kingdom, Mellaart would identify the invaders as Luwians. To support this assumption and his chronology, he points to the clay seal with Hittite hieroglyphic inscription found in level VI at Beycesultan[17] and the small limestone obelisk discovered at Byblos in Syria, which has the Luwian name *Kuhun* referred to as the son of the *Lukka* man. Beycesultan VI is assigned to Early Bronze III, the Byblos stele is dated as early as the XIIth Dynasty of Egypt.[18] Thus it would appear that while the ancestors of the Greeks were invading Greece, the Indo-European Luwians were settling in western, southern, and southwestern Anatolia and perhaps Cilicia.

Mellaart assumes that a culture distinguished by gray Minyan pottery developed in the regions south and east of the Sea of Marmora and in the Iznik-Tavsanli area. This culture supposedly spread westward during Early Bronze III. Its bearers would have established Troy VI and the Minyan culture of Middle Helladic Greece.[19] This theory, which was rejected by scholars on various grounds such as the absence of Greek place names in northwestern Anatolia, now breaks down in view of the Lerna evidence, which suggests that gray wares must go back before Early Helladic II, i.e. before Early Bronze III in Anatolia.

Surveys in western Anatolia have turned up gray wares in Early Bronze III context around Isnik and inland as far as Tavsanli and Bozuyuk, suggesting that the old chronology which assigned ordinary gray ware to Troy V and Minyan gray ware to Troy VI is meaningless.[20] Mellaart is possibly right in postulating a movement from east to west in Anatolia during Early Bronze III which was marked by the destruction of sites such as Karaoğlan in central Anatolia. When Karaoğlan was reoccupied at the beginning of the Middle Bronze Age, a pottery appears which has its best connections with that of Kültepe Ib, which is dated to the time of the latest level of the Karum, or trading center, of the Assyrian merchants.[21] The tablets from the later levels of the Karum offer only slim evidence for assuming that the Hittites must be associated with the movement from east to west at the end of Early Bronze III. Much more archaeological work is needed in eastern Turkey before one can fix the character of this period, to say nothing of the movements of peoples. Nevertheless one is tempted to connect the Anatolian and north Iranian gray wares and to see a gradual penetration of peoples into Anatolia paralleling the movement into northern Iran. The early stages of such a movement may well have dislodged the people who carried the Khirbet Kerak pottery southwards to Cilicia, Syria, and Palestine during the Early Bronze Age.[22]

Present archaeological evidence clearly indicates two periods of destruction in northwestern Anatolia which may well reflect movements of peoples on the steppe to the north. The first movement, associated with the destruction of Troy I, was relatively limited in its effects, which seem to have been restricted to western and northwestern Anatolia and Macedonia. The second was far more serious. This movement, which is marked by the destruction of Troy II and the towns of southwestern, southern, and southeastern Anatolia as well as Cilicia, must in some way be related to the movement into Greece indicated by the Early Helladic II destruction horizon. One can only speculate about the character of the movement from the east at the end of the Early Bronze Age which Mellaart would seek to associate with the Hittites.

The discovery of the Early Helladic II destruction horizon, the redefinition of the character of the Early Helladic III period, and the back-dating of the period when the horse was introduced into southwestern Asia and perhaps the Aegean, together with the reevaluation of the significance of the Troy I and II destructions, make it necessary to reconsider the dating of the dispersal of the Indo-Europeans. This reconsideration is forced by new historical and archaeological evidence, quite aside from the pressure for drastic chronological changes brought by carbon-14 dates.

A great number of European cultures have been associated with the first dispersal of the Indo-Europeans. First of all there are the Corded Ware and Battle-Axe cultures of central Europe, the Single Grave culture of Scandinavia, the Kurgan culture of southern Russia, and the Fatyanovo culture of the woodlands of central Russia. Second one must include cultures such as the Baden of the Middle Danubian Basin and find groups

such as that of Decea Mureşului in Transylvania, which have been linked to the cultures of the steppes of southern Russia. There are also finds of ochre graves, corded ware, and battle-axes from the southern Balkans and corded ware finds from Greece. Everywhere the tumulus-kurgan type of tomb, contracted burials (often lying on ochre), pottery with cord-impressed decoration and battle-axes have been taken as the hallmarks of cultures usually associated with the early Indo-Europeans in Europe.

Until recently these cultures were assigned to a narrow chronological horizon dated to the early second millennium B.C. Obviously such a concept of dating rested upon the assumption that the dispersal of the Indo-Europeans could not date very much before the appearance of Minyan pottery in Middle Helladic Greece, the invasion of the Hittites and the Hyksos movement. The revision upward by four or five hundred years of the dating of Minyan pottery and of the Luwian and Hittite invasions suggests that the Corded Ware and Battle-Axe horizon in Europe must go back toward the middle of the third millennium B.C.

In Europe this shift of dating is clearly supported by carbon-14 dates but, if this is accepted, one is faced with very long time spans for the Corded Ware-Battle Axe cultures and their subsequent local developments, which lasted down to the Early Bronze Age horizon of central Europe.[23] Archaeological work, however, has long indicated that many of these Corded Ware cultures may have had relatively long life spans. The Saxo-Thuringian culture, the first with pottery defined as Corded Ware, had a long period of development, which can be subdivided into two typological phases partially supported by barrow stratigraphy.[24] These two phases, the earlier Kalbsrieth and the later Mansfeld, began at the time of the Salzmünde Funnel Beaker culture, paralleled the Walternienburg-Bernburg, Globular-Amphorae and Bell Beaker cultures and terminated with the spread of Aunjetitz elements.[25] Although there are no carbon-14 dates for samples from Saxo-Thuringian sites, related Dutch and Scandinavian carbon-14 dates, combined with a high but widely accepted date for Aunjetitz (Bronze Age A 1), indicate that the Saxo-Thuringian culture should be dated from about 2500 to 1800 B.C.[26] The pottery of the early phase is characterized by broad-bodied beakers with flat bases and amphorae, both decorated with vertical and horizontal cord impressions, simple stroke patterns, and rows of small pits. They should date somewhat later than the Dutch protruding-foot beakers, which have less well developed forms and a simpler decoration. It has generally been assumed that the Corded Ware culture spread from central Germany into Hannover, the Lower Rhine valley and the Low Countries, suggesting that the Corded Ware cultures of these areas are later than those of central Germany. However, the simpler forms and decoration of the protruding-foot beaker suggest that at least the Dutch group was earlier than the Kalbsrieth group of central Germany. It is possible, however, that only a few bands of Corded Ware peoples were able to penetrate into central Germany during the earlier phases of their expansion because the people of the Salzmünde

culture, who controlled the salt wealth of the Saale valley and possessed fortified settlements such as Dölauer Heide near Halle, could have turned back any invaders.[27] A few corded ware sherds found in Salzmünde context suggest that the earliest Corded Ware peoples did penetrate into central Germany. Full-scale settlement, however, came shortly afterward, with the Kalbsrieth group. Barrows like those of Bohlen-Zschwitz and Burgorne indicate that the Kalbsrieth group was contemporary with the Walternienburg culture.[28] The location of Walternienburg settlements on heights which were probably fortified suggests that they existed in troubled times. The later phase of this central German culture paralleled the Bell Beaker culture of central Europe and survived until the beginning of the Aunjetitz age.

The Corded Ware culture of the Netherlands does not derive directly from the Saxo-Thuringian but from West German Beaker groups. Only five examples of the large Saxo-Thuringian amphora have been found in the Netherlands. The development of the Dutch Beaker culture can be traced through five phases, but only the first two concern us here, since the later phases are entirely local in character and involve fusion with Bell Beaker groups. The first phase was characterized by the protruding-foot beaker, which has been dated by carbon-14 determinations made for samples from Anlo, Schaarsbergen and Ede to 2470 ± 55 B.C. (old half-life) or 2602 ± 55 B.C. (new half-life), 2485 ± 320 B.C. (old half-life) or 2618 ± 320 B.C. (new half-life), and 2245 ± 120 B.C. (old half-life) or 2370 ± 120 B.C. (new half-life) respectively.[29] The second phase, which is defined by the all-over-corded beaker and beaker with bands of herringbone decoration, is typologically later, a relative dating supported by a carbon-14 date from Anlo of 2190 ± 70 B.C. (old half-life) or 2314 ± 70 B.C. (new half-life).[30]

Archaeological evidence from the Rhineland, southwestern Germany, Switzerland, and Austria is not sufficient to define the Corded Ware phases. The Swiss corded ware and certainly the corded ware of the Austrian Single Grave culture, as well as that reported from the Laibacher Moor in Slovenia and farther south in Yugoslavia, are for the most part very late in date.[31] The *Litzen-keramik* of Austria and the northwestern Balkans must date to the eve of the Early Bronze Age.

In Czechoslovakia the development of the Corded Ware culture has been subdivided into three phases by the Neustupnýs.[32] The earliest phase is known only from stray finds, which include beakers comparable to the earliest of central Germany and the Netherlands. The pottery of the second phase is comparable to that of the Kalbsrieth group, while that of the third phase is similar to the pottery of the Mansfeld group. The Neustupnýs point out that the first phase was contemporary with the Řivnáč culture, which does not belong to the Corded Ware tradition, and that the second phase, marked by the arrival of new Corded Ware groups from the north, was at least in part contemporary with the latest Řivnáč. The destruction of the hilltop settlements of the Řivnáč culture probably

took place toward the end of the first phase of the Corded Ware culture in Bohemia.[33] Ehrich points out that the earliest Řivnáč has Cannelated elements and that developed Řivnáč has elements suggesting contact with the Bernburg (contemporary with Walternienburg), Vučedol and Globular Amphorae cultures, indicating that Řivnáč continued into the second Corded Ware phase in Bohemia.[34] A carbon-14 date of 2438 ± 70 B.C. (new half-life) (for which I am indebted to R. W. Ehrich) dates charcoal from the burnt floor of a house at Homolka which belongs to a late phase of the Řivnáč culture. The Homolka date must mark a period before the destruction of the Řivnáč culture, at the latest a phase contemporary with the second Corded Ware phase, and indicates that the Kalbsrieth group of central Germany and certainly the earliest Corded Ware of Bohemia should date well before 2400 B.C.

In Poland the earliest Corded Ware phase is known from a number of find groups, many of which are difficult to fix chronologically. The earliest Corded Ware is known from only a few finds such as those reported long ago from Gross Morin in the district of Inowroclaw in northwest Poland and from Lukawice in the district of Lubaczow in southeastern Poland. While these beakers cannot be firmly dated, they are entirely similar to the earliest Bohemian and Dutch beakers.[35] The development of the Corded Ware culture of Poland is generally traced through four phases in terms of the Złota and Rzucewo cultures.[36] The beakers and amphorae of the Złota I culture are entirely comparable to those to those of the Kalbsrieth group and the second phase of the Corded Ware culture in Bohemia. The second, third, and fourth phases of the Złota culture followed a somewhat different course of development from that of the Corded Ware culture in Bohemia and central Germany. Finds from the fourth Złota phase suggest that it was contemporary with the Early Bronze Age of central Europe. Other Corded Ware groups, such as the Marszowice, or Marschwitz, and Great Polish-Masovian, or Oder, groups, had vessels of flower pot shape similar to those of the Late Single Grave culture of Denmark and seem very late in comparison to even the mature phases of Bohemian, central German, and Dutch Corded Ware cultures.[37]

Today there is every indication that the Corded Ware cultures of central Europe lasted a long time. Carbon-14 dates suggest that they began as early as 2600 B.C. and lasted down to the beginning of the Bronze Age of central Europe. Everywhere there are indications that this development fell into three, possibly four, stages. It may well be that the earliest Corded Ware cultures, the Dutch, Bohemian, and earliest Polish, which might go back as early as 2600 B.C., mark the first penetration of the Corded Ware peoples. They would have moved across Poland and penetrated into central Germany, where they may well have been turned aside by the Salzmünde people. The Dutch protruding-foot beakers indicate that they reached the Netherlands, while stray finds suggest that they intruded into Bohemia, where the land was falling into the hands of the peoples of the Cannelated

Ware and Řivnáč cultures. Shortly afterward stronger groups pushed westward from the steppes to establish the Złota I culture in Poland, the Kalbsrieth group in central Germany and the second phase of the Corded Ware culture in Bohemia. Although there are regional differences due to survival of older cultural elements, they have in common similar beakers and amphoras which are decorated with horizontal and vertical cord impressions, simple stroke patterns, and rows of pits. Such pottery has its best affinities with that of the Yamno (Pit Grave) or Kurgan III phase of the Pontic steppe.[38] The Dutch carbon-14 dates, together with the Homolka date and inferential dates for the Walternienburg culture (with which the more developed phase was certainly contemporary), suggest that the earliest Corded Ware as well as the somewhat later developed phase defined by the Kalbsrieth group and the Złota I culture should be dated between 2600 and 2400/2300 B.C.

The mature phase of the Corded Ware cultures, such as the Złota II culture of Poland, the Mansfeld group of central Germany, the third phase of the Corded Ware cultures of Bohemia, and the all-over-corded Beaker phase of the Netherlands marked the beginning of their dominance in central Europe during an age usually associated with the Bell Beaker traders. While the Złota culture continued into its second phase in Poland, the Mansfeld group completely displaced the Walternienburg culture in central Germany in the same way that the later Corded Ware culture of Bohemia put an end to the older cultures. In the Netherlands the fusion of Corded Ware and Bell Beaker groups was already moving in the direction of the later Zoned Beaker cultures. Everywhere pottery displays increasingly local forms, but these later Złota, Mansfeld, and Bohemian cultures have a pottery distinguished by the common use of triangular and angular motifs. This type of decoration has close analogies with that of the Kurgan IV pottery on the Pontic steppe.[39] Chronologically, the later Corded Ware cultures began about 2400/2300 B.C. and lasted down to the beginning of the Early Bronze Age of central Europe.

The onset of the mature Corded Ware cultures must have been marked by contact with the steppe, perhaps even by the arrival of new groups from the Pontic steppe. As M. Gimbutas rightly points out, most of the Corded Ware finds can be equated in time with the Kurgan IV phase, but it is important to note that the similarities are between Kurgan IV pottery and mature Corded Ware pottery, not the pottery of the earlier Corded Ware horizon.[40]

The Single Grave culture of Denmark and southern Sweden has strong ties with the Corded Ware cultures of central Europe and the steppe and must be regarded as intrusive in Jutland, the Danish Isles, and also in southern Sweden despite Malmer's attempt to seek a native origin.[41] There is sufficient contact between the Single Grave culture of the interior and the coastal Megalithic Passage Grave culture, the chronology of which can be established, to date the intrusion of the Single Grave peoples into southern Scandinavia. Unfortunately there are only a few burials to mark the

beginnings of the Single Grave culture, which leads to some disagreement over the exact time of its arrival in Jutland in terms of the chronology of the Passage Graves. Glob has placed its arrival just after the older phase of the Passage Grave culture, presumably in Middle Neolithic II, on the basis of evidence from Mejls near Varde. Here there is a passage grave with an assemblage dating only from the older phase of the Passage Grave culture. Afterward it was deserted, but in the area there are tumuli with the earliest type of Single Grave assemblage, allowing one to speculate that the arrival of the Single Grave peoples forced out the Passage Grave peoples. On the other hand, Becker argues that present field evidence does not permit one to place the Single Grave culture earlier than Middle Neolithic III.[42] The chronology of the Single Grave culture of Denmark is based upon barrow stratigraphy, typology and connections with the Passage Graves. It is generally assumed that the Bottom Graves date to late Middle Neolithic III and early Middle Neolithic IV, the Ground Graves to late Middle Neolithic IV and early Middle Neolithic V, and the Upper Graves to late Middle Neolithic V.[43] The Swedish Single Grave, or Boat-Axe, culture, which has been subdivided into Continental, Early and Late phases, roughly paralleled the Danish Single Graves, although some archaeologists arguing for a different origin place its beginnings a little later. The Danish Passage Grave phases have external connections which make it possible to establish their relative chronology in relation to northern Germany and beyond. It is important to note that as far as the current literature indicates, Middle Neolithic II, which immediately antedates the Single Grave culture in the Becker chronology, is usually regarded, on the basis of pottery, as contemporary with the Walternienburg culture.[44] This synchronism is of great importance since the Walternienburg connection indicates that at least the earliest Corded Ware cultures must date before the Single Grave culture. It must be noted, however, that the form and decoration of the beakers of the Bottom Grave phase have their best analogies with continental beakers of the early Corded Ware cultures. This suggests that the people who brought the Single Grave culture to Jutland were of the older Corded Ware horizon. Although archaeological evidence is limited, there is some evidence which suggests that the Single Grave people penetrated into Denmark during the period of the early Corded Ware cultures. This evidence consists of stray finds of the earliest type of battle-axes (type I). Their presence in the river valleys leading from the North Sea into the interior of Jutland and in central Jutland, though not in eastern Jutland or the Danish Isles, where the Passage Grave culture would have been dominant, suggests that Single Grave people could have penetrated into western Denmark during the period of the older Corded Ware culture. Unfortunately, there are no closed finds to support such an hypothesis.[45] Middle Neolithic III, the period when the Single Grave culture made its full appearance is south Scandinavia, is usually dated to the mature Bell Beaker horizon of central Europe, a date which fits well with the chronology of the Danish Single Grave culture. The numerous foreign connections of Middle

Neolithic V indicate that the Single Grave culture lasted down to the time of the Late Neolithic cist graves, i.e. to the eve of the Bronze Age in central Europe.

There is general agreement on the broad correlation of the phases of the Danish and Swedish Single Grave cultures. Controversy, however, besets the question of the origin of the Swedish culture. Many archaeologists would derive it from Denmark and account for marked differences in pottery style on the basis of cultural fusions in Sweden, while others such as Malmer seek an entirely local origin. Sturms has pointed out that the Swedish Single Grave culture, the Fatyanovo and the Dniepr-Desna cultures have pottery forms, with rounded or pointed bases, distinct from those of the central European Corded Ware cultures and the Danish Single Grave culture, and make much less use of cord-impressed decoration. According to Sturms, this suggests that the Swedish Single Grave culture had a different origin and may well have reached Sweden via a more northerly Baltic route.[46]

As we have seen, the evidence from central and northern Europe as well as that from the Aegean and southwestern Asia suggests that there were two major periods of expansion from the Eurasiatic steppe. One of these must be placed ca. 2600 B.C., the other around 2400/2300 B.C. Before turning to the evidence from the steppe itself, a brief evaluation must be made of new finds from Rumania and Serbia to determine the effect of these two expansions upon cultural development in southeastern Europe.

In Bulgaria, corded ware is found at Michalic in a context clearly datable to Troy I–II,[47] and pottery with cord-impressed decoration, as well as a clay copy of a battle-axe, is reported from level VII (V) at Karanovo,[48] but unfortunately the stratigraphy is inadequate at both these sites. It is as difficult to evaluate chronologically as the Corded Ware finds reported from Greece. Unfortunately the only really certain evidence for corded ware in Greece consists of a single sherd found in a context dated toward the end of Early Helladic III at Eutresis.[49]

Rumanian finds of corded ware (which Gimbutas has rightly connected with the Kurgan or Yamno IV phase) are largely reported from Cucuteni B context, but they are also known from sporadic Rumanian kurgan finds.[50] Indications of the penetration of steppe influence are also evinced by the Gorodsk-Usatovo elements found in the Cernavoda I culture, the rise of the Coţofeni culture and the Decea Mureşului graves, but all these are difficult to fix in terms of relative chronology. Some of these difficulties have been cleared up by the recent excavations at Gumelnitza. Here Dumitrescu found both crusted and graphite wares firmly fixed in Gumelnitza B (III) context, wares which had previously been known no later than the Gumelnitza A2 (II) phase. With the crusted and graphite wares there were two imported vessels with painted decoration which must be associated with the Cucuteni A3 phase.[51] This discovery indicates that major revisions must be made not only in Gumelnitza chronology but also in that for the closely correlated Sălcuţa and Petreşti cultures. A prelimi-

nary evaluation which indicates new chronological correlations suggests that Gumelnitza B (III) must now be correlated not only with Sălcuţa II but also with Sălcuţa III, indicating that the later Gumelnitza B (III) and Sălcuţa III had much longer development than was previously thought. Without tracing here the technical implications and details of the Gumelnitza discovery, it enables us to understand the fourth-millennium carbon-14 dates for the Sălcuţa and Cucuteni cultures.

The rise of the Coţofeni culture in Oltenia and Transylvania provides the best evidence for phasing the penetration of steppe elements into southeastern Europe. Coţofeni elements are first found in Sălcuţa IV and Petreşti IV contexts, which may be equated with the last phase of the Gumelnitza culture and the Cernavoda I culture, thanks to the revision in chronology permitted by the recent excavations at Gumelnitza. Connections with Karanovo VII, in Bulgaria, and the Aegean area as well as indirect connections with the Bubanj-Hum Ib culture in Serbia suggest that these cultures date to Troy II and Early Helladic II times.[52] Level 7 at Piscul Cornisorului has a Sălcuţa IV assemblage in which there appeared pottery forms foreshadowing Coţofeni forms,[53] and at Tărtăria one can trace a steady increase in Coţofeni elements in level IV, which is still dominated by a Petreşti assemblage.[54] It is important to note that the fully developed Coţofeni culture finally appears in level 8 at Piscul Cornisorului, which is stratified immediately over level 7, and that the upper layers of level IV at Tărtăria yield full Coţofeni with both Baden and Kostolac elements, a fact of importance for Baden chronology. Aside from other evidence, the development of culture at these two sites suggests that there was a phase marked by a westward drift of steppe elements followed by a phase characterized by a much larger movement of steppe elements. Carbon-14 dates of 2495 ± 100 B.C. (old half-life) or 2628 ± 100 B.C. (new half-life) and 2310 ± 100 B.C. (old half-life) or 2437 ± 100 B.C. for the Cernavoda-Ezero I culture, which can be correlated with the earlier phase of infiltration, give us some idea of its absolute dating.[55] The Decea Mureşului graves of western Transylvania, which Gimbutas has placed in Kurgan III times, have Bodrogkeresztúr connections, suggesting that they also belong to the early phase.[56] It is difficult to place the ochre graves of Baia-Hamangia on archaeological grounds. Carbon-14 dates are conflicting, since the Berlin laboratory gives a date of 2140 ± 160 B.C. (old half-life) or 2262 ± 160 B.C. (new half-life), only a little later than the old Groningen date of 2464 ± 65. B.C. (new half-life), but the corrected Groningen date is 2580 ± 65 B.C. (old half-life) or 2715 ± 65 B.C. (new half-life).[57]

These same two phases mark the development of the Baden culture in the Middle Danubian basin.[58] The beginning of the Baden culture, which has long been thought to have a strong steppe component, must now be placed relatively early in the third millennium, owing to corrections made in the Bubanj-Hum chronology. These corrections rest upon the check excavations made by M. and D. Garašanin in the 1950's at Bubanj and also upon the more recent excavations at Hissar in Serbia. Chronological

corrections were necessitated by the discovery that the Pseudo-Minyan wares used to fix the Bubanj-Hum chronology must be pushed back from Middle Helladic I to Early Helladic III.[59] Briefly, the Aegean change allows the Bubanj-Hum chronology to rise, while the Hissar excavations suggest that Baden-Kostolac elements do not occur until Bubanj-Hum II and that the Bubanj layers II–IIa, which have Baden-Kostolac elements, should probably be differentiated from layers III-3 and assigned to Bubanj-Hum II. On this basis layers III-3 at Bubanj should be attributed to Bubanj-Hum Ib. This makes it possible to accept a two-phase concept for the development of the Baden culture, which is in line with Jovanović's interpretation of the 2.50-0.60 meter level at Vinča as well as with evidence from Hungary.[60] The Early Baden phase, which developed in western Hungary during the period when the Bodrogkeresztúr culture dominated eastern Hungary, should probably be correlated chronologically with the early phase of the penetration of steppe elements into southeastern Europe. The Anatolian traits of the Baden culture, such as forms of burial, wheeled vehicles and pottery decoration among others, warn us that the Baden culture may have come from the southeast, and hence may have been only indirectly connected with movements out of the steppe. The Late Baden was everywhere marked by Kostolac elements which occur without Baden associations only in Bosnia. One might speculate that the movements associated with the second expansion from the steppe ca. 2400/2300 B.C. dislodged the Kostolac culture from Bosnia, pushing it into western Hungary where it merged with Baden to form the Baden-Kostolac characteristic of Late Baden times.[61] Such a theory is supported by the chronological position of the Baden-Kostolac elements found in Bubanj-Hum II context, now dated in terms of Early Helladic III.

The problem of the chronology of the steppe cultures has been purposely put off until the end of this paper. The relative chronology proposed by Marija Gimbutas for the Neolithic and Chalcolithic cultures of the area north of the Black Sea provides a basis for my explanation of the role of the steppe in the development of cultures in surrounding areas.[62] However, chronological schemes for cultures of Anatolia, the Aegean, the Balkans, central Europe and Scandinavia, which rest upon both archaeological and radiocarbon evidence, clearly suggest that the absolute dating of the Yamno (or Kurgan) III phase must be somewhat earlier than proposed by Gimbutas. Her chronology rests upon the equation of the phases of the Yamno culture with the Nal'chik, Maikop, and Tsarskaja phases of the culture of the northern Caucasus, which she dates in terms of perfectly good, but late, connections in the Near East. While the Nal'chik phase, which is equated with early Yamno III, has few southwest Asiatic connections, the Maikop phase, which is correlated with late Yamno III, is tied to Alaca Hüyük and Höröztepe. Höröztepe is dated to 2200 B.C. and the Alaca Hüyük Royal Tombs somewhat earlier, between 2400 and 2200 B.C. Mellink notes in her recently published Anatolian chronology that the Alaca Royal Tombs should be placed in full Akkadian times and equated

with Troy IIa–g.[63] It can be argued that Troy II, the Alaca Royal Tombs, and therefore the Maikop tombs should go back to the time of the Royal Tombs of Ur, i.e. to the Early Dynastic III period. In the northern Caucasus, this dating is supported by the evidence from the tombs at Kabardino Park, which are contemporary with the Maikop tombs. The Kabardino tombs yield pins similar to those of Troy II-III and Alaca Hüyük which have prototypes in the mouflon- and spiral-headed pins current in southwestern Asia as early as Susa D, Ninevite Chagar Bazar and Hissar II.[64] These southwest Asiatic connections for the Maikop phase suggest that the third phase of the Yamno culture may well go back to the beginning of the third millennium. There is no need to alter drastically the absolute chronology of the Yamno IV phase, but it probably begins somewhat earlier than proposed by Gimbutas.

If one accepts a revision of the absolute dating of the Yamno III phase, the development on the Pontic steppe fits perfectly with that of adjacent areas. Movements out of the steppe during Yamno III were at first still limited, as suggested by Gimbutas, but they were very far-ranging, reaching as far west as the Netherlands. The earliest type of beaker found in the Netherlands, in Bohemia and in Poland suggests an initial penetration by small groups of Corded Ware peoples. Not long afterward, but still during the period of the Yamno III culture on the steppe, larger groups established the Złota I culture of Poland, the Corded Ware II culture of Bohemia, and the Kalbsrieth group of central Germany. Present evidence does not permit one to say whether the peoples of the Single Grave culture had reached southern Scandinavia by this time. While all these groups were moving northwestward out of the Pontic steppe, other groups pushed westward to account for the Corded Ware elements found in the late Tripolye-Cucuteni cultures of Moldavia and the western Ukraine, the elements anticipating the Coţofeni culture found in Sălcuţa and Petreşti context, and the formation of the Usatovo-Gorodsk culture. These movements must have been paralleled by the movement of the Baden culture into the Middle Danubian Basin, but it is difficult to establish where it came from or its route of migration. Many traits such as 'horses, wheeled vehicles, cattle and sheep, a dairy economy, patriarchial families, and bows and arrows' may be assigned to the early Indo-Europeans on the basis of linguistic palaeontology.[65] Some of these traits are Anatolian and argue for an Anatolian route.

Whatever the origin and route of the Baden culture, the northwestward movement of the Corded Ware culture into central Europe and the westward drift of steppe elements into the Balkans seem to have begun as early as 2700/2600 B.C. Certainly these movements must have been connected with the fall of Troy I and were not unrelated to the movement of the Gray Ware culture into northern Iran. It is unfashionable to resort to climatic or environmental explanation, but it must be noted that the transition from the moist, warm Atlantic to the dry, cool Sub-Boreal climate took place in northern and northwestern Europe about 3150 B.C.[66] The curves of pollen

diagrams suggest that Sub-Boreal conditions became increasingly intense as the third millennium passed. The first expansion out of the steppe may well have come when it was no longer possible for the mixed farming-herding communities of the steppe to maintain their old ways. The strong herding component of their economy made mobility possible, and as a consequence the peoples of the steppe could move away, at first in small groups, and then in larger ones, to the northwest and north and, undoubtedly, southward as well into the Near East.

By 2400/2300 B.C. the Corded Ware groups were beginning to consolidate and achieve dominance in many areas of Europe, the Aegean, and the Near East. In central Europe, this may be seen in the Złota II culture, the Corded Ware III culture of Bohemia, and the Mansfeld group of central Germany. Often older cultural groups were absorbed, which would explain the intense particularism of the later Corded Ware groups. Along the Middle and Lower Rhine and in the Netherlands the Corded Ware peoples fused with the Bell Beaker peoples, creating the distinctive late Beaker cultures. This same process of absorption and fusion of cultures can be seen in the later phases of the Single Grave culture of Scandinavia. Nevertheless, the distinctive pottery decoration of these late cultures suggests contact with the Yamno IV culture of the steppe. While some of the similarities in decoration may have been due to the spread of fashion, they can also be interpreted as due to the movement of new groups out of the steppe. The consolidation of the Coţofeni culture in the Balkans, coupled with the movements behind the destruction of Troy II and the related destruction of towns in Greece and Anatolia, implies that there was a large-scale expansion to the west and south, if not the northwest, around 2400/2300 B.C.

Many questions remain unanswered, but the solutions to many of our problems require further excavations. The question of the origin and relationships of the Gray Ware cultures of Greece, Anatolia and Iran and the puzzling questions concerning the Baden culture can only be answered through further field work. Above all, there is the question of the correctness of the chronology presented in this paper, but this answer will be known only after a through testing of its implications.

Notes

[1] Gro-330. H. de Vries, G. W. Barendsen, and H. T. Waterbolk, Groningen radiocarbon dates II, *Science* 127 (1958) 135. In this paper the radiocarbon dates have been converted for the new half-life by multiplying the dates determined in terms of the old half-life by 1.03. See H. Godwin, The half-life of radiocarbon 14, *Nature* 195, no. 4845 (Sept. 8, 1962) 984.

[2] V. G. Childe, *The Dawn of European Civilization* (New York, 1948), new 4th ed., chart on p. 333.

[3] R. Pittioni, *Die urgeschichtlichen Grundlagen der europäischen Kultur* (Vienna, 1949), chart on p. 100.

[4] GrN-1855. J. Vogel and H. T. Waterbolk, Groningen radiocarbon dates IV, *Radiocarbon* 5 (1963) 180. Some archaeologists have reservations about the Anlo date since it dated somewhat earlier than was expected. Nevertheless dating of the

earliest phase of the Corded Ware culture to 2400 B.C. (i.e., 2530 B.C. when supporting carbon-14 dates are corrected for the new half-life), proposed by H. T. Waterbolk or as early as 2600 B.C. as proposed here, fits with other radiocarbon dates and archaeological evidence for the dating of the Corded Ware cultures found elsewhere in Europe. See H. T. Waterbolk, *The Lower Rhine Basin. Courses toward Urban Life* (New York, 1962) 242.

[5] J. L. Caskey, Excavations at Lerna, 1957, *Hesperia* 27 (1958) 125–44.

[6] Childe, *op. cit.* 65–6.

[7] J. L. Caskey, Greece, Crete, and the Aegean islands in the early Bronze Age, *Cambridge Ancient History* I, ch. XXVI(a) (Cambridge, 1963) 16–7.

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[26] V. G. Childe, *The Dawn of European Civilization* (New York, 1958) 6th ed., 342.

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[28] Mildenberger, *op. cit.* 26–7.

[29] See above for Ede (Gro-330) and Anlo (GrN-1855). Gro-318 (Schaarsbergen):

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Some Indo-European Speaking Groups of the Middle Danube and the Balkans: Their Boundaries as Related to Cultural Geography Through Time

Robert W. Ehrich

This paper is an incomplete draft of a segment of a much wider study that is now under way. For some years I have been developing the thesis that culture areas and culture boundaries have a tendency to persist through time if there is a long-standing cultural tradition; and, if such a long tradition is absent, that they keep reappearing despite the influx of new peoples with different cultures (Ehrich 1956; 1961; 1965:404ff. see also Jażdzewski 1948:12–13, '. . . whether it is admissible and fair to correlate historic ethnic areas with territories of prehistoric cultures. . . . I believe that we may and should favourably view the thesis . . .'). In this regard it is necessary to restate that 'culture' and 'culture area' are both sliding-scale terms that can be used on various levels of abstraction, applicable to any group with shared cultural traits from the village or tribal level to wide reaches of territory in which a general similarity can be perceived. In this sense the term 'culture' has the meaning of the total way of life of a given people at a given time. The size of the group or the area which it occupies must be specified in order to have such meaning, and any comparisons, to be valid, should be made between units on the same level of classificatory abstraction.

Since archaeological data can provide only a limited sample of the total content of the culture of any particular group, it becomes necessary to deal with the diffusion patterns and boundaries of individual traits which may seem particularly significant for interpretation. Thus pottery, because of its ubiquity as a locally manufactured trait, usually of no basic significance beyond its actual use, and because of its short life and inherent complexity as a trait, is often a more sensitive index in defining a group with a shared esthetic tradition than are tools, weapons, or ornaments of metal which, although possibly just as complex, were rarer, more highly prized, or were perhaps trade objects that reached beyond the confines of a group's territory.

The purpose of this paper is to examine some geographic patterns of Indo-European speaking groups of Iron Age peoples of East Central

Europe and the Balkans and to see whether their territories and boundaries coincide with those of other periods. My total study has not yet advanced far enough for me to ask why such boundaries should be recurrent, although in certain cases the answer seems fairly obvious. This paper is still part of the initial process of testing whether the continuity or recurrence of patterns can be established sufficiently well so that an inquiry into the reasons that lie behind each such case, with perhaps some generalized assessments of pertinent cultural dynamics, may be justifiable.

The area here treated (Fig. 1) takes in present-day Yugoslavia, most of Hungary and Transylvania, eastern Rumania and the Lower Danube Basin, with some attention to west Bulgaria and northeastern Greece. Archaeological exploration has by no means been uniform over this territory, and the information with regard to the southernmost tier—which extends from Istanbul to the Adriatic and includes Thrace, Macedonia, northern Albania, and Montenegro—is even less than sketchy. The definition of any culture area boundary, whether a line or a zone, presupposes another culture area beyond it. Since in this study the emphasis is on the limits of various areas rather than on the areas themselves, no single culture area is completely delineated.

Furthermore, since many Iron Age peoples can apparently be recognized from historical sources and since there seems to have been a general level of civilization throughout the Iron Age, one might find some term other than 'culture area' more acceptable, although regional differences are recognizable. One is tempted to resuscitate Holmes' (1903) 'geo-ethnic' areas for application to such a situation, despite the fact that he applied the term and concept in the exact sense that Wissler later used 'culture area'. It must be further remembered that culture boundaries in the sense here used are not impenetrable, that isolated occasional groups may intrude into neighboring territory, and that particular traits and trait complexes may diffuse independently by contact rather than by migration.

Gavazzi (1956:p. 7 and note 11, pp. 10–12), writing on the modern ethnology of Yugoslavia, quite rightly stresses that the divisions between culture areas tend to be zones of admixture of greater or less width, rather than sharp lines on a map. This point was made by Wissler in his early writings on the culture area concept (1917; 1923) and later by Dixon (1928:24, 25), Goldenweiser (1937:477–78), Kroeber (1939:5, 6), and others.

Perhaps our best point of departure is the territory of the Illyrians as discussed in a symposium held at Sarajevo in 1964 and published there in that same year as *Simpozijum* (Symposium sur la délimitation territoriale et chronologique des Illyriens à l'époque préhistorique). In this volume the distinctions are clearly drawn between Illyrian speech, Illyria, and the Illyrians, much as the title of the present symposium distinguishes between Indo-European and Indo-Europeans.

If we accept the Adriatic as the western limit of the Illyrian people, we can begin with the paper by Marić (1964) defining their northern boundary.

In his discussion and on his two maps following p. 213 (Figs. 6, 7, below), he establishes the line between the Pannonians and Illyrians as that between the Urnfield cultures to the north and the Tumulus culture (Hügelgräber) to the south, and he finds that this coincides closely with the boundary as given by Strabo. As mapped by both Marić (1964) and Cović (1964) (Figs. 6, 7, 8, 9, below), this line does not quite follow the watershed, but cuts across the headwaters of the Vrbas and Bosna, and lower down across the Drina, all northward-flowing tributaries of the Sava River. In this regard we should mention that in geologically young landscapes the headwaters of streams lie in sharply cut, narrow, ravinelike valleys, generally unsuitable for farming communities. The more open valleys lower down on such tributaries invite newcomers to push up from the main river to a point where the hills close in and the relatively rich valley bottoms pinch out. It is at this point that the topography imposes a different pattern of life, and it is quite common to find ethnic groups extending over even rugged watersheds to this extent. Although modern political boundaries (like that between Poland and Slovakia) may follow the crests of divides, in places where water control or large-scale defense is not important, the dividing line between cultures is more likely to separate different adaptations, such as mountain herding or transhumance on the one hand and a settled valley type of life on the other. (In this instance, suggested informally by Dr. A. Benac, and with reference to the modern ethnology of the Resava River Valley, a tributary of the Morava, by Dr. Milenko S. Filipović. See also V. Popović in COWA 6, III, 1966.)

The eastern boundary of the Illyrians seems less definite. Čović, in his paper on what he calls the Central Illyrian region, shows the north boundary as crossing the Drina near Rogačica at its bend, curving to include most of the West Morava and then turning southeast to run parallel along the left (west) bank of the Upper Morava and returning westward to take in virtually all of the Drina drainage, reaching the coast below the Mat. (Figs. 8, 9.) This is, of course, quite schematic.

Garašanin, in writing on the eastern boundary of the Illyrian area (1964), is not precise but locates it along the Morava and upper Vardar basins rather than along the rivers themselves. He divides Serbia into two strips, West Serbia which is clearly Illyrian, and the Morava Valley and eastern Serbia which is somewhat different. In discussing named groups as well as the archaeological finds, he places the Dardanians to the north and the Paeonians to the south and other tribes in between along the rivers (Fig. 5). Using historical as well as archaeological sources, he reaffirms that they are mixed groups, seeing them as predominantly Illyrian but with strong Thracian admixture that increases during the course of the Iron Age. He emphasizes particularly that the metal work is of Illyrian character. However, more recently (Garašanin and Nestor, 1966) he seems to consider them as predominantly Illyrian. (See addendum, item 5, p. 233 below.) In this regard the paper on Krivi Dol near Radanje (Garašanin, M. and D. 1959) is of particular interest, for this Hallstatt burial site lies east of the

Vardar and is almost certainly in the area of the Paeonians. Garašanin describes the burial types as being mainly Illyrian in character, and the metal objects as special local Macedonian variants of the Illyrian types characteristic of Bosnia. On the other hand, the pottery types are related to those from eastern Macedonia to the southeast, e.g. Vardarophtsa, as compared with the West Macedonian painted wares of Boubousti type. It would seem more likely that metal types and metal technology would be diffused, perhaps through the movements of a few craftsmen, than would domestically produced, handmade pottery which would be manufactured by a larger sample of the population on a home industry basis and which would thus presumably have been a more stable culture element. The historical record shows that, in general, religious acculturation and the spread of burial types is not unusual.

Whether these groups are actually Thracianized Illyrians, or Illyrianized Thracians as sometimes thought, makes little difference for our purposes. It seems reasonably clear that the drainage of the upper Morava and the greater part of the Vardar formed a border zone of admixture lying between the territories of the Illyrians and Thracians, and Čović's demarcation of the eastern edge of the central region of the Illyrians, although schematic, is probably reasonably accurate. His southern boundary seems less certain, primarily because of an even greater lack of exploration in that area, but on geographical grounds may well anticipate what will appear during the course of further archaeological work.

We now reverse our steps and comment on these boundaries with relation to other periods and other data. In southwest Macedonia, the Pelagonian Plain near Bitolj is drained by the Crna which ultimately flows into the Vardar. The plain itself extends southward to the Greek border and beyond, and it is not surprising that in Neolithic and Bronze Age times this area was open to influences from Thessaly (Fewkes 1934:50ff.; Grbić 1956; Garašanin 1958:123–25; Ehrich 1965). The topography would also have permitted relatively easy access for the penetration of later Greek elements found in the Lake Ohrid area at Trebenište and other sites not yet published (Garašanin 1958). Although not yet adequately explored, it seems quite possible that the Pelagonian Plain served as a contact and mixed border zone in more than one period, and that the southern edge of the Illyrian cultural center should fall here.

Along the eastern side, Fewkes, Goldman, and I (Fewkes, Goldman and Ehrich 1933, Fewkes 1934) argued years ago that to call the Vardar-Morava valley a cultural highway was a misinterpretation of its role, and I have since referred to it briefly as a zone or area of composite culture and an area which absorbed independently diffused traits (Ehrich 1954; 1956; 1961; 1965). The discussion of its role in Thraco-Illyrian times repeats much of what can be said of the Neolithic. In both Stone Age and Iron Age, the open Morava Valley seems to have been a southward extension of the Danube pattern with the boundary somewhere along its eastern side and, to some extent at least, this may have continued some distance down the

Vardar. Certainly below Demir Kapije (the Iron Gate of the Vardar), and probably well north of it, the mixed character becomes clearer. The site of Slatina at Zelenikovo near Skopje, as published, seems clearly Starčevo (Galović 1964) but neolithic sites near the Bregalnica seem mixed in character, apparently with some Starčevo and particularly Kremikovci traits, e.g. Amzibegovo. (Personal communication from the late J. Korošec. See also Korošec and Saržoski 1960 and R. Galović 1966.) Just where one should draw the eastern boundary of this mixed zone is something of a question. It should probably lie at least as far as the Timok and Struma drainages, with the next area extending to the Isker. This last statement is based primarily on geographical possibilities, for adequate archaeological evidence is lacking. According to G. I. Georgiev (personal information), neolithic materials of Kremikovci type are found throughout the Sofia basin but to date have been found extending eastward only in a few caves along the northern slopes of the Stara Planina. South of the range one finds the Karanovo complex in the Maritza drainage, providing a close fit with Cvijić's (1918) division of modern cultural types (Fig. 2).

In his discussion of the contemporary ethnology of the Balkan peninsula as a whole, Cvijić found some major differences in culture patterning which he laid in part to survivals from the Illyrians on the west, Thracians on the east, and an intervening zone of admixture. He further commented on the differences in the ethnic composition of the western and eastern Slavs. Also significant is his statement (p. 20) that the Ovče Polje-Strumica route was of major importance even at the time he wrote, that it had been the chief Byzantine highway, and that until the construction of the railroad along the Vardar it was the primary communication line and that the gorges of the Vardar were consistently shunned. He further adds that the Turks did not follow the Morava too closely.

Although Cvijić uses parts of the Danube, Sava, Drava, and part of the Drina rivers to demarcate his culture types and varieties, his mixed central areas includes the Vardar and Morava drainages, its eastern boundary falling along the watershed on the east side of the Isker and including the separation of the Isker-Maritza drainages. Gavazzi (1956), following Cvijić's divisions to a great extent, tends to use the main watersheds also, rather than the rivers themselves. Although his map (Fig. 3) does not correlate exactly with archaeological boundaries, it does approximate them reasonably well. Thus the boundary between his Pannonian and Dinaric culture provinces lies south of the Sava, but not so far south as Marić and Čović put it (see Figs. 7, 8, and 9). Of further interest is Dr. Gavazzi's characterization of the area around Skopje as an isolated area (personal information), implying that the central zone not only comprises some groups with cultural admixture, but that its mixed character is something of a mosaic as well. In another personal discussion, Dr. Milenko S. Filipović pointed out that, despite the ethnic diversity of the various peoples in much of Macedonia, observable culture traits were largely held in common. This is, of course, a common phenomenon of culture area dynamics.

Moving westward along the north border, we have seen that beyond the Drina the Illyrian boundary as shown by Marić and Čović generally follows the watershed line of the southern Sava drainage but lies slightly to the north of it, conforming to a change of terrain. In a sense this can be considered as something of a fall line. On their maps, Marić (1964), Čović (1964), and Gavazzi (1956) all show a continuous line running roughly parallel with the Sava. It seems worth suggesting that a more detailed plotting might show a more jagged effect, with tongues extending southward up the valleys and the hills between them relatively empty but conforming to the southern pattern.

With regard to other cultures, the archaeological picture is not yet clear. However, the northward-flowing Bosna and Drina seem to have served as concentration zones for different neolithic groups. South of the Sava, Starčevo material occurs at Gornja Tuzla on the Spreča, between the Drina and the Bosna (Benac 1962:Fig. 3). This also is apparently the westernmost extent of the Vinča culture (Benac 1959). Remembering always that the neolithic patterns of Bosnia, Herzegovina, and Dalmatia, as mapped by Benac (1962), are probably incomplete due to variations in exploration, the following distributions are highly suggestive. For the neolithic cultures, the Starčevo and Vinča sites lie east of the Bosna (Benac 1959; 1962:Fig. 3, p.67), whereas the Butmir settlements cluster along the valleys of the Bosna drainage to the Sarajevo area.* The various neolithic groups extend upstream to the north Illyrian line as shown by Marić and Čović, and the eneolithic sites pretty well repeat this pattern with an additional cluster on the Sana (Benac 1962:Fig. 7, p. 145; also Fig. 10, below).

The area between the Sava and the Drava is not yet thoroughly explored but seems to form something of a buffer zone between the northern and southern areas. Just how far to the west the Starčevo and Vinča complexes extended is something of a question, but, at present at least, the dividing line between Syrmia (Srem) and Slavonia seems to mark the approximate limit. Starčevo material has been found near Osijek at Sarvaš (Schmidt 1945:127–31) and generally along the Osijek-Vukovar-Vinkovci line, as have some Vinča elements. In Slavonia some linear traits and some Lengyel pottery (e.g. at Babska) reflect at least a partial southward penetration from western Hungary, which later was the Pannonian area (personal information from S. Dimitrijević).

West of Zagreb, Cvijić (1918:510) attributes the general cultural character to the Slovenes as ancient Slavic invaders, and east of Zagreb to ancient immigrants of Balkan Dinaric background. East of Zagreb, across Slavonia to Vinkovci, he sees the population as largely composed of Roman Catholic immigrants from Bosnia and Herzegovina, whereas east of Vin-

* As of 1969, Starčevo and Vinča materials have reportedly appeared in the Valley of the middle Bosna. It is not yet clear whether these represent trait diffusion, population admixture, or penetration of the area by one or more village settlement groups.

kovci, in Syrmia and the Banat, are Serbian Orthodox immigrants from Kosovo, Metohija, and Morava-Vardar Serbia. The general correspondence of these divisions with the pattern of neolithic distributions, now apparently emerging, is striking: i.e. Alpine, west of Zagreb, Transdanubian between Zagreb and Vinkovci, and the Starčevo-Vinča complexes of the Danube-Tisza-Morava plain east of Vinkovci (e.g. Fig. 12, below).

I have already emphasized the importance of the Adriatic watershed as something of a cultural divide (Ehrich 1961, 1965) and, with the exception of two Danilo-Kakanj sites on the upper Bosna and one Hvar-Lisičiči site on the upper Drina, this pattern seems to hold. Since the rivers of the interior flow north, it is not surprising to find cultural differences arranged in north-south strips along their valleys. West of the divide the drainage is, of course, generally westward and, so far at least, most of the known neolithic and eneolithic sites lie north of the Neretva, although there are a few exceptions. Whether this situation merely indicates lack of sufficient investigation in this southern part, or whether the very narrow coastal strip was uninviting and impractical for hand-tool subsistence-farming settlement is not yet clear. In any event, it is of distinct interest that Čović (1964: Maps II and IV, Figs. 8, 9 below) shows Illyrian flat graves and Gorica type ornaments as clustering north of the Neretva and west of the watershed, in contrast to the tumuli and Glasinac types of the interior. Here again there are exceptions, but the general distribution suggests an internal subdivision of the Illyrians, with this North Dalmatian region being something of a cultural subarea. If we compare this with more recent evidence, we have only to remember that major Romanized cities stretched along the Dalmatian coast, that veterans were settled there and along the Neretva, and that even today Dalmatians have an ethnic character somewhat different from that of the people of the interior, partly due, of course, to continuing seaborne influences across and up and down the Adriatic. In this regard Čvijić (1918:298) stresses the importance of the Adriatic watershed as a dividing line. (See also Gavazzi 1956.)

To sum up the situation, the north boundary separating the Tumulus culture (apparently that of the Illyrians) from the Urnfield culture (apparently of the Pannonians) seems to have followed the line crossing the various river valleys, thus dividing the terrain into downstream portions of the valleys that were suitable for farming with the techniques then available from the headwater areas that were fit only for pasturage, thus conforming more closely to the needs of the pastoral Illyrians. Along the west coast the recognizable northern section was at least topographically distinct as a subarea, and to the east the buffer zone of the Morava-Vardar drainages not only separated two major ethnic areas with a zone of admixture, but was itself divided into northern and southern ecological as well as topographical regions which are also reflected in the persistence of cultural patterning.

To the northwest, according to Gabrovec (1964:238), the Illyrian pattern borders on the Southeast Alpine Hallstatt culture, which he divides

into five groups. Of these, Unterkrain (Lower Carniola) is characterized by tumulus burials of the Illyrian complex. This group reaches to the Magdalenska Gora, almost to Ljubljana, where its western boundary is very sharp, and this type of burial is not known west of this point nor north of the Sava; in the east it extends to the junction of the Krka with the Sava, and to the south runs as far as the Kolpa (Kupa).

The rest of the picture is not too clear, but the Southeast Alpine zone, not surprisingly, seems an area of blending in which some elements and influences came from Italy, some from the south, and others moved westward up the Sava and Drava, particularly the area between them, and up the Mur. The cultural patterning is thus one of various admixtures, and seems to repeat both the situation and distribution that characterize the mixed neolithic East Alpine facies known as the Lasinja culture, found in Carniola, Carinthia, Styria, and western Croatia. (See Ehrich 1965:420 for references to J. and P. Korošec.) In this case, however, there are strong influences from the south and west as well. In general the Illyrian territory does not extend into the East Alpine Hallstatt zone and, on the basis of archaeological differentiation, Gabrovec (1964:246) recognizes central and western Balkan Iron Age provinces and considers that the territories of such later groups as the Japodes, Istrians, and Liburnians should not be reckoned as properly Illyrian.

Dealing with modern ethnography, Gavazzi (1963) sees the region of the upper Kupa, generally Gorski Kotar in the Dinaric Alps above Rijeka, as the meeting place of four distinct culture patterns: Alpine, Adriatic, Dinaric, and Pannonian (Fig. 4).

North of the Illyrian-Thracian tier, the Carpathian Basin also furnishes long-standing geographic cleavage lines between cultural groups. Here we begin with the two Roman *limes* systems, of which the first divided the Pannonians on the west from the Sarmatians in the center, and the second separated the Sarmatians from the Dacians on the east. The first of these merely follows the line of the Danube on its southward course, dividing the hilly country of Transdanubia from the Great Hungarian Plain. (See Fig. 5 for this and for following discussion.)

As mapped by Kiepert (n.d. Map XII), the *limes* then traverses the Danube-Tisza strip below the Drava-Danube and Maros-Tisza junctions, runs northward along the mountainous western edge of Transylvania, cutting across the Maros and Körös, and then curves eastward, roughly paralleling the upper Tisza but south of it, presumably near the Körös (Criş)-Tisza watershed, to cross the upper Someş. More specific data is given by Szilágyi (1943, 1946) who lists a series of *limes* sites and plots the western defense line of Dacia as running north from the Danube to the Someş (1943:91).

Protase (1962) analyzes some 72 burial sites which date from the La Tène period to the fourth century of the present era. Taking differences in burial type as good indicators not only of Romanization and lack of it but as clues to the boundaries of Dacia, he (p. 175) sketches them as extending

eastward along the north bank of the Maros from Jánosszállás near the Maros-Tisza junction and then as turning northeastward through the Apuşeni massif and across the upper waters of the Crişul Negru and the Someş. The Tisza and the plain to the east of it seem definitely to lie in what was Sarmatian territory. The occasional Roman camps and forts in this area must thus be regarded as outposts or advance lines which probably shifted over the course of time. The western *limes* of Szilágyi, in part at least, would seem to provide the effective frontier between the Sarmatians to the west and the Dacians to the east. The line then follows the inner frame of the northeastern and southeastern Carpathians and returns due south to join the Danube somewhat to the east of the Olt. This last stretch closely approximates the *Limes Transalutanus* as mapped by R. Vulpe (1961:374) who also shows the somewhat earlier *Limes Alutanus* as following the course of the Olt. In this regard it is worth noting that the Olt drainage, through time, plays a customary role as a culture boundary zone but now with political and military overtones; and even today the Olt forms the division between Oltenia and Muntenia.

Also of interest is the *vallum* line stretching eastward from the Iron Gate to skirt the fall line of the streams flowing south from the Transylvanian Alps to the Danube. In a recent article Horedt (1965a) argues that this and also the *valla* at the western fringe of the Transylvanian highlands date from the fourth to the eighth centuries of the present era. In any event, as defense systems they follow the terrain which also has some significance with regard to cultural distributions. (See also Balás 1963.)

(The term 'Transylvania,' as here used, designates only the geographic entity of the mountain-ringed basin and does not include the plains of the Banat and of Crişana-Maramureş.)

The significant topographic features which divide this upper tier into geoethnic units, all running north-south, are, from west to east: the eastern edge of the Alps, the course of the Middle Danube, the Tisza Plain, the western edge of the Transylvanian Mountains, the course of the upper Olt, the course of the middle and lower Olt, the eastern edge of the Carpathians, and the plateau and plains to the east, marked into strips by the Siret, Prut, Dniester, and Dnieper (Fig. 1). The present paper does not pursue the analysis to its full eastern extent.

With regard to the Neolithic, I have pointed out (Ehrich 1965) that the Danube-Tisza strip was relatively uninhabited until Bodrogkeresztúr times, that it separated the Linear (Zseliz) and Lengyel traditions on the west and north from those of the Körös (Criş) and Tisza cultures to the east, and that the Tisza, Tisza-polgár, and Herpály cultures extended eastward to western Transylvania. Unpublished Linear pottery, apparently early, is now said to occur in considerable quantity in northeastern Hungary in the plains area to the north of the Körös and south of the Tisza. (Personal information kindly supplied by N. Kalicz and R. Tringham.) In the absence of precise information on distributions, maps or otherwise, it is difficult to be certain, but this report does suggest that these Linear

sites lie to the northwest of the Dacian *limes*. They almost certainly fall in the next culture area to the north, which would include eastern Slovakia where similar finds are now appearing (Fig. 11). Although the Bodrogkeresztúr sites occupy the strip between the Danube and the Tisza, the Danube separates them from the Lengyel tradition of Transdanubia, and to the east they again penetrate to western Transylvania (Fig. 13). The subsequent Hatvan and Otomani complexes also seem to fall into what was later to become Sarmatian territory. It is of interest that the two complexes that extended across the entire Middle Danube Basin, the Baden and the secondary Vučedol (Makó and Nyirség) groups, also covered the later Sarmatian area but not that of the Dacians.

According to Patay (recent personal information), his maps of Early Bronze Age distributions, although somewhat out of date (1938), are still reasonably valid for the purposes of this paper. More sites have, of course, been discovered in the area shown and new ones have indicated that some of the groups extended farther to the south than was known at that time. A summary of his maps in tabular form, using Transdanubia, the Danube-Tisza strip, the Tisza River with its eastern lowland drainage (including Crişana-Maramureş), and Transylvania as four distinctive geographic provinces and ignoring an occasional penetration beyond their confines, gives the following pattern:

	Transdanubia	Danube-Tisza Strip	East Tisza Plain	Transylvania
Bodrogkeresztúr	−	+	+	Very slight (4 on upper Maros)
Baden	+	+	+	−
Vučedol	+	+	+	−
Nagyrév	Kulcs group	Kötöres type Ökörhalom type	+	−
Kisapostag	+	−	−	−
Perjámos	−	−	+	−
North Pannonian Incrusted	+	Slight		
South Pannonian Incrusted	+	+	Slight	−
Vatya	Slight	+	−	−

In this sense the regionalization and boundaries are quite clear. The distributions of South Pannonian Incrusted are essentially western but stray finds extend across the northern half of the Danube-Tisza strip and along the Tisza, while the Bell Beaker, Oggau, and Gata complexes cluster in the Rába drainage of northwest Hungary, and Magyarad lies only along the north arm of the Danube elbow. As described by Bóna (1963), the Nagyrév cemeteries are not really Transdanubian. Two of his groups (Kulcs and Szigetszentmiklós) occupy a relatively narrow strip running southward from Budapest, mostly along the west side of the Danube; a

third (Ökörhalom) reaches to the upper bend and is also found on the Tisza north of the Körös; and a fourth (Kötöres) stretches not only along the Tisza between the Körös and Maros, but also along the Danube westward from the bend. It is also worth remarking that Dušek's map of the North Pannonian Incrusted (1960:212) conforms quite closely with that of Patay, and supplements it. None of these cultures or groups penetrates Transylvania, where the sequences are markedly different.

During the Bronze Age the Danube again serves as a cultural boundary, with the Urnfield Vál complex, which succeeded the Tumulus culture, occupying the territory to the west, and the Csorva complex that to the east (Trogmayer 1963). These seem to represent long-standing traditions developing more or less parallel to each other from the Late Bronze Age (Reinecke BD) at least into Hallstatt B. Trogmayer's map (p. 115) shows the Csorva sites to be scattered thinly along the east bank of the Danube and across the center of the Danube-Tisza strip, and to be clustered heavily at the Maros-Tisza junction. Others, not shown, were reportedly found in the Dacian area to the east. It is the west boundary that is of major significance here. He sees some clear differences between the western sequence, which has urn burials and some turban bowls, and the eastern Csorva, which has inhumation as well as urn graves and some characteristic pottery forms such as pseudo-Villanovan double-conical vessels that are not typical in the west. Thomas (1965), in discussing the distribution of turban bowls, finds them initially in the west Hungarian Tumulus group of the Bronze Age, and subsequently in all phases of the Vál culture.

Köszegi (1960:181) gives the west Hungarian sequence as a Bronze Age Tumulus culture replaced by Vál I (an Urnfield culture equating with Velatice in Czechoslovakia and Baierdorf in Austria), followed in turn by Vál II, which is contemporaneous with the Czechoslovakian Podoli and the Austrian Stillfried complexes.

His rough datings as modified by S. Foltiny (personal information) are:

Surviving Tumulus Culture I (Bronze Age D) begins about 1250 B.C.
Surviving Tumulus Culture II (Hallstatt A_1) begins 1200/1150—
Vál I (Hallstatt A_2) begins 1100/1050—
Vál II (Hallstatt B) = 950/900–750/700
True Iron Age (Hallstatt C) = 750/700–600
Hallstatt D = 600–450/400

Thraco-Cimmerian elements appear in Hallstatt C, Scythian in Hallstatt D. Quite pertinent is the view of Patek (1961), that in Transdanubia survivals of the Tumulus culture last into Hallstatt C and that influences from the east were relatively slight as compared with those from the East Alpine area to the west, implying a stable and rather isolated population. This also reinforces Köszegi's point that a marked differential existed between Bavaria, where the Urnfield cultures began in the thirteenth century, and the Carpathian Basin.

Early La Tène (Reinecke La Tène A) seems to be absent from the Hun-

garian Basin, but Reinecke La Tène B, apparently Celtic, appears at approximately 380 B.C. The Scythian irruption and occupation probably preceded but may have overlapped that of the Celts. Párducz (1965:map, p. 275) shows the Scythians north of Lake Balaton to the Danube, west of the Danube, and in the central and northern parts of the Danube-Tisza strip. He quotes Fettich (1928) to the effect that three groups of them were located in Transylvania, the Tisza region, and west of the Danube respectively. He himself combines the Danube-Tisza and northern Transdanubian distributions into a single group with a gap between Lake Balaton and another strong cluster in Slovenia to the south. Another group, that of Kustánfalva, lies to the northeast in the Carpatho-Ukraine. Foltiny (1963) thinks the question as to whether these people were ethnically Scythians or Scythianized Thracians is of little importance, but that the significant point is that their remains represent an incursion of steppe people. He sees them as strong in Slovenia, having had some influence in the Burgenland, but very weak in lower and upper Austria, Styria, and Carinthia. In other words, they washed across the middle Danube Basin to the edge of the Alps. To the north, Dušek (1961) considers the so-called Scythian remains to be those of acculturated Thracians.

Correlatively, Hunyady (1944) shows no Celts in the center or south of the Danube-Tisza strip nor between the upper Maros and the upper Olt at the time the Celts occupied both central and northeast Transylvania. Horedt (1965b) lists 17 sites in Transylvania in which Middle La Tène wheel-made pottery is found in conjunction with handmade pottery of an older Hallstatt tradition. He dates this complex as approximately second century B.C. and considers it to mark a brief Celtic incursion which was absorbed by the Dacians who had apparently been in the area since Hallstatt times. Although Roska (1944:155) maps 147 Celtic sites in Transylvania, more than 120 of these are stray finds, and of the rest most are graves or hoards, with only two listed as settlement sites. The presence of Celts in the area was certainly of short duration.

In analyzing the results of his excavation at Moreşti, Horedt finds some Poieneşti elements which he thinks diffused down the Maros from their home territory west of the Siret in the eastern Carpathian foreland of Moldavia and in southeast Transylvania east of the Olt. Conversely, Párducz (1956:Fig. 2, p. 19; see also Foltiny 1966) maps Dacian finds that extend to the west as occurring along both banks of the Tisza, a few scattered in the Danube-Tisza strip, and a definite cluster at the Maros-Tisza junction that stretches northward toward the Körös. He shows the Sarmatians as being concentrated in three areas with some thinly scattered sites between (1950). These areas of greater density are the center of the Danube-Tisza strip, the northern part of the strip and the Tisza Valley above the Körös junction, and along the Körös River itself. The Dacian remains west of the Tisza he considers as dating from the time of Burebişta. Beyond the scope of this paper is the northward extension of Dacian finds in both eastern and western Slovakia, as mapped by A. Točik (1959). In

like vein, Todorović (1962) traces a one-handled conical cup form which he takes to be a special Dacian type characteristic of the Banat. He thinks that the distribution of this type beyond the Banat area, as far south as Paracin on the Morava and as far west as Hrtkovci on the Sava, may also be correlated with the Burebişta expansion.

Despite the evanescent appearance of Celts in Transylvania, in general terms one can describe them as lying west of the Danube and the Dacians as east of the Tisza. In Roman times the Celts in the hilly lands of Pannonia (Transdanubia) were Romanized, the plains of the central strip and east of the Tisza to the mountains were controlled by the Sarmatians (Párducz 1950:80, 108, 109, 113, 115), and the Dacians occupied Transylvania. In this regard it is significant that, according to Szilágyi (1946), R. Vulpe (1961), and Protase (1962), the Roman province of Dacia included Transylvania and Oltenia but not Muntenia (Fig. 5).

In the eastern section of this second tier a fairly clear set of geographic patterns is emerging. Surrounded by mountains and forming a major topographic unit, most of Transylvania drains directly westward to the Tisza across the lowlands of Crişana-Maramureş and the Banat via the Mureş (Maros) and Criş (Körös) river systems, and in the north and northwest via the Someş, which is also a part of the Tisza complex. The Olt, which is the only river to cut into Transylvania from the south, marks off southeast Transylvania as a separate region. To the east the Bistriţa, an eastern tributary of the Siret, is the only significant breach in the eastern Carpathian wall.

In addition to delineating divisions within Transylvania, these rivers have also served as routes of penetration and diffusion, as shown by the clustering in some distribution patterns. The upper waters of the Mureş, for example, finger out, the sites on the northern branches often indicating contact with the upper Someş while the population on the southernmost arm was characteristically oriented toward the Olt. It is not surprising, therefore, to find a western complex, such as that represented by the few Bodrogkeresztúr sites on the Mureş (Patay 1938; Vlassa 1964), apparently indicating a people who had moved up the Tisza, around and up the Someş to reach across the middle Mureş, rather than having entered Transylvania by way of the main stream of the Mureş itself. The group of sites on the upper Mureş, given by Párducz (1954:55) as belonging to or related to the Vekerzug group, again may well have entered the area by way of the Tisza or Someş. It is also of interest that Chişvasi-Comşa (1958:82) maps the Slavs identified by the Luka Raikovitkaia-Hlincea I culture of the seventh to ninth centuries as ascending the Dniester and descending the Tisza, with one branch turning to go up the Someş and then across the watershed to the middle Mureş, apparently much as the Bodrogkeresztúr group had done some millennia before them. Conversely, clusters of sites on the upper Olt may have a few related ones on the southern branch of the upper Mureş, as, for example, those of Boian (Comşa 1962), Proto-Cucuteni (Roman 1963), and Cucuteni. The Olt, too, seems to have served for limited pene-

trations of elements that followed it upstream and across the mountains but did not spread widely within the basin. A good example is Horedt's map (1960b) of intrusive Mycenaean elements which form a group on the upper Olt and upper Mureş. According to Berciu's rather schematic presentation (1961:235), the lowland Verbicioara culture of the Bronze Age also penetrated northward along the Olt as far as the bend.

Except for the inclusion of southeast Transylvania and the headwaters of the Bistriţa, Moldavia sometimes forms the western fringe of a cultural territory that may extend as far as the Dnieper. Particularly clear in this regard are the distributions of the Cucuteni-Tripolye complex (Passek 1962; Dumitrescu 1959:6, 1963:285; Gimbutas 1956, 1965) and that of Noua-Sabatinovka (Florescu 1964). Gimbutas (1956:101) shows the Cucuteni-Tripolye sites as falling in the forest-steppe zone, and Usatovo sites in the steppe to the south of it. Although the Noua sites extend to the Dniester, they lie only in the forest-steppe, while the Sabatinovka sites eastward to the Dnieper occur only in the steppe zone (Fig. 14).

The distinction between the Moldavian and general Transylvanian patterns is constant. A minor exception seems to be the Sîntaná de Mureş-Cherniakhov culture of the fourth century, the burials of which are most heavily concentrated in Muntenia but are significantly, if thinly, represented in Moldavia and on the upper Mureş (Diaconu 1964).

Despite a scattered handful of Linear sherds (Comşa 1959), the Central Transylvanian sequence is characterized first by the Criş (Körös) complex and then by Vinča-Turdaş, Petreşti, Coţofeni, Furchenstich, Wietenberg, possibly Noua [mapped here by Florescu (1964:190) as diffused elements only), Scythian, and ultimately by the Dacian Iron Age. Horedt (1960a: Fig. 4, p. 110) maps Wietenberg sites as concentrated mainly on the upper Mureş and upper Someş, to some extent on the upper Olt, but not significantly to the southeast of it. The bronze hoards from Bronze Age D through Hallstatt C seem western in character and, as mapped by Rusu (1963) for seven phases, their distribution shows virtually nothing in the corner to the southeast of the Olt. Protase (1962:195), however, includes this area within the confines of Roman Dacia, while Bakó (1961) locates the Bijelo-Brdo cultures of the eleventh through thirteenth centuries in central, but not in southeastern Transylvania. He interprets this as indicating that the Magyars and Germans did not at first penetrate this southeast corner, which he thinks was occupied by an autochthonous population who had links with the Slavs and Byzantines farther to the southeast on the Lower Danube and on the Rumanian Plain.

The Moldavian sequence, on the other hand, while showing some Criş elements, is marked by late Linear Wares (Novotný 1959: Pl. 1; Tchernych 1962), Boian, the Precucuteni and Cucuteni-Tripolye and Folteşti series (Comşa 1962; Dumitrescu 1963:285; Gimbutas 1956; Passek 1962; Roman 1063:35), globular amphorae (Dinu 1961), Monteoru IC3 in the south (A. Vulpe 1961:107; 1964a:map p. 134; 1964b:328; Florescu 1964:211; Bichir 1962:114) and Costişa in the north, followed by Noua (Florescu

1964; A. Vulpe 1964b), Poienești—attributed to the Germanic-speaking Bastarnians by R. Vulpe (1955) and Horedt (1965a) but to the Thracian Carps by Bichir (1961)—and then by the Sarmatians who were partly contemporary (Bichir 1961; Ioniţa 1964). For the later periods, Chişvasi-Comşa (1958) believes the older Romanized population and the southern Slavs to have moved northward from the Danube, around the Carpathians into Moldavia, although she indicates that this interpretation is more hypothetical than securely fixed.

Southeastern Transylvania usually belongs to the cultural territory of the plateaus and plains beyond the mountains, either to the southeast in northeast Muntenia, or to the east in Moldavia, or it shares in the complexes of central Transylvania. The general character of this region through time may thus be viewed as that of a small, unstable, cultural subarea of the type that I have described as one of varying affiliation (Ehrich 1956:8; 1961:254). Beginning with the Neolithic, its sequence apparently starts with Criş (Central Transylvanian, Muntenian, and to some extent Moldavian), and is followed by Vinča-Turdaş (Central Transylvanian and Muntenian), some Boian on the upper Olt and Mureş (Muntenian), the Precucuteni and Cucuteni series (Moldavian), Coţofeni (Central Transylvanian and Oltenian), Schneckenberg-Glina III (Muntenian, Oltenian) (Bichir, 1962:90), Tei (Muntenian), Wietenberg (Central Transylvanian) (See Bichir, 1962:114 for table; E. Comşa 1962 for map of Neolithic; and Ehrich 1965 for summarizing treatment.)

In the lower Danube Valley, Starčevo-Criş may lie at the base of both sequences, extending eastward as far as the Siret (e.g. Teodorescu 1963: 268), but subsequently the major Oltenian series runs: Vinča-Turdaş, Vădastra, Salcuţa, Coţofeni, Glina III, Verbicioara; while that of Muntenia reads: Boian, Gumelniţa, Cernavoda, Glina III, Tei, and Monteoru. In general the division is provided by the Olt drainage south of the mountains which later (Protase 1961; R. Vulpe 1961) formed part of the boundary of Roman Dacia. The cultures of each sequence appear to a greater or lesser extent across the Danube to the south, either as closely related complexes such as Kremikovci and Krivodol in northwest Bulgaria vis-a-vis the Oltenian Starčevo-Criş and Salcuţa cultures respectively, or as distributions of single cultures, such as Vădastra. Opposite Muntenia, Boian sites lie north of the Stara Planina and those of Gumelniţa not only extend from the Osam to the Black Sea but southward into the Maritza drainage with a couple of outliers on the Tundzha as well.

Although the Iron Gate of the Danube does operate occasionally to separate complexes of Oltenia from those of the Banat and other lowlands to the west of it, in a surprising number of instances the Oltenian cultures can be identified with those to the west. Starčevo-Criş, Vinča, Coţofeni appear in both regions during the Neolithic. For the Bronze Age, Berciu (1961:235) shows the Periám (Perjámos)-Mokrin-Pančevo complex extending eastward to the upper Jiu (as well as to the Mureş on the north), and the distribution of the Vatina-Gîrla Mare culture east of the Iron Gate

as lying close to the north bank of the Danube with a suggested wider spread in Northwest Bulgaria. Conversely, the Verbicioara culture is concentrated primarily in Oltenia, occurs south of the Danube in northwest Bulgaria and, in an early phase, in a slight penetration to just west of the Iron Gate. (For a generalized mapping of the Neolithic distributions dealt with in the entire discussion see Figs. 11–14.)

Although the evidence is still sketchy and the mapping schematic, it seems worthwhile to point to a few internal divisions which may be appearing. Northeast Muntenia frequently seems to go with southern Moldavia, and it is of interest to note that Comșa (1963) maps the northern border of the Aldeni II-Stoicani complex, which is located here, as cutting diagonally northeastward from the southeast corner of the Carpathians across the lower Siret and lower Prut to the Dniester, while Izvoare (Precucuteni III) lies north of this line. (See also Roman 1963.) The eastern *valla* as shown by Horedt (1965b) and R. Vulpe (1961) are also suggestive. To some extent there may be some repetitive divisions between north and south Moldavia (see, for example, A. Vulpe 1961:107 for distributions of Monteoru and Costișa), while the Ialomița on more than one occasion seems to have been the limit of southward penetrations or of trait diffusions (e.g. Teodorescu 1966:225 for map of Linear wares).

SUMMARY

It has been the purpose of this paper to explore some aspects of the geographical distributions of certain Indo-European groups in the southern part of east central Europe. Taking Indo-European speaking groups known from historical sources, I have attempted to test whether they conform to the thesis that the geographical patterning of an area tends to be stable or recurrent through time as one examines a series of cultural maps of different periods. Since it is virtually impossible to discuss the limits of a given culture area in these terms without going on to inquire what lies on the other side of them, I have concentrated more on selected culture boundaries than on complete culture areas in order to avoid opening a new Pandora's box with each successive boundary described. It is for this reason that I have not attempted to close off the Pannonian or Transylvanian areas on the north nor the Moldavian, Dniester, and Dnieper areas on the north and east. Our review has covered not only the Illyrian territory, with emphasis on its northern and eastern bounds and with subsidiary attention to the mixed border zone of the Morava-Vardar drainages, to the area of admixture in the southeastern Alps, and the coastal drainage area north of the Neretva, but also Transdanubia, the Danube-Tisza plains, Transylvania, Moldavia, and the lower Danube Basin from the Early Neolithic to Roman, Medieval, and even to some extent to modern times. It seems clear that topographic, physiographic, and ecologic factors have served continuously as isolating mechanisms in the regionalization of culture, and that, although they may

have been temporarily obliterated during periods of instability and population movement, they have tended to reassert themselves repeatedly during the course of time. The Indo-European speaking peoples of prehistoric and protohistoric times were subject to these processes, and their cultural distributions seem to conform well with those demonstrable for the culture history of the area as a whole.

Addendum

Attention should be called to an important contribution (M. Garašanin, and I. Nestor, 1966) that appeared just as this article was ready for the press, in time for citations to be included but too late to incorporate extended critical comment in the body of the text. Since the authors orient their discussion around geographic zones, it is not surprising that part of what they say coincides closely with some points mentioned above. From the standpoint of this paper items of particular interest are:

1. Their correlation of Starčevo with Karanovo I–II, and Vinča with Karanovo III, as showing regional distinctiveness, accentuated by their description of Kremikovci in the Sofia plain and Vršnik in the Bregalnica area as intermediate (p. 23).

2. Their mention of the Timok or Nišava valleys as the probable route by which the Bubanj-Hum group entered the Morava area (p. 24).

3. Their division of the Bronze Age cultures into three major groups—Eastern, Western, and Northern (Balkans and Carpathia), the northern being subdivided into local groups, i.e. Vattina-Vršac in the Banat; Dubovac-Žuto Brdo in the Banat, Danubian Serbia, northwest Bulgaria and Oltenia; Verbicioara in Oltenia; Wietenberg and Otomani in Transylvania; Tei partly in Transylvania but mostly in Valachia and Bulgaria; Monteoru in North Muntenia and South Moldavia; and Noua (somewhat later) in Moldavia and eastern Transylvania (pp. 27 and 28).

4. Their statement that, although there was strong Hellenization along the Adriatic coast, Hellenic influences did not penetrate eastward into the interior.

5. Their description of the Vardar-Morava zone as the general division between Thracians and Illyrians but with the recognition of regional fluctuations (p. 33).

6. Their adherence to the idea of a Vardar-Morava cultural highway (p. 20). My own comments on the role played by the Vardar and Morava systems (pp. 219–221 above) take a different point of view.

7. Their simple labeling of the finds from Krivi Dol at Radanje near Štip as Illyrian. Following the evidence as presented in the paper on the site itself (M. and D. Garašanin, 1959) I have treated them as a Paeonian assemblage of mixed cultural elements. This still seems to me to be a more probable interpretation.

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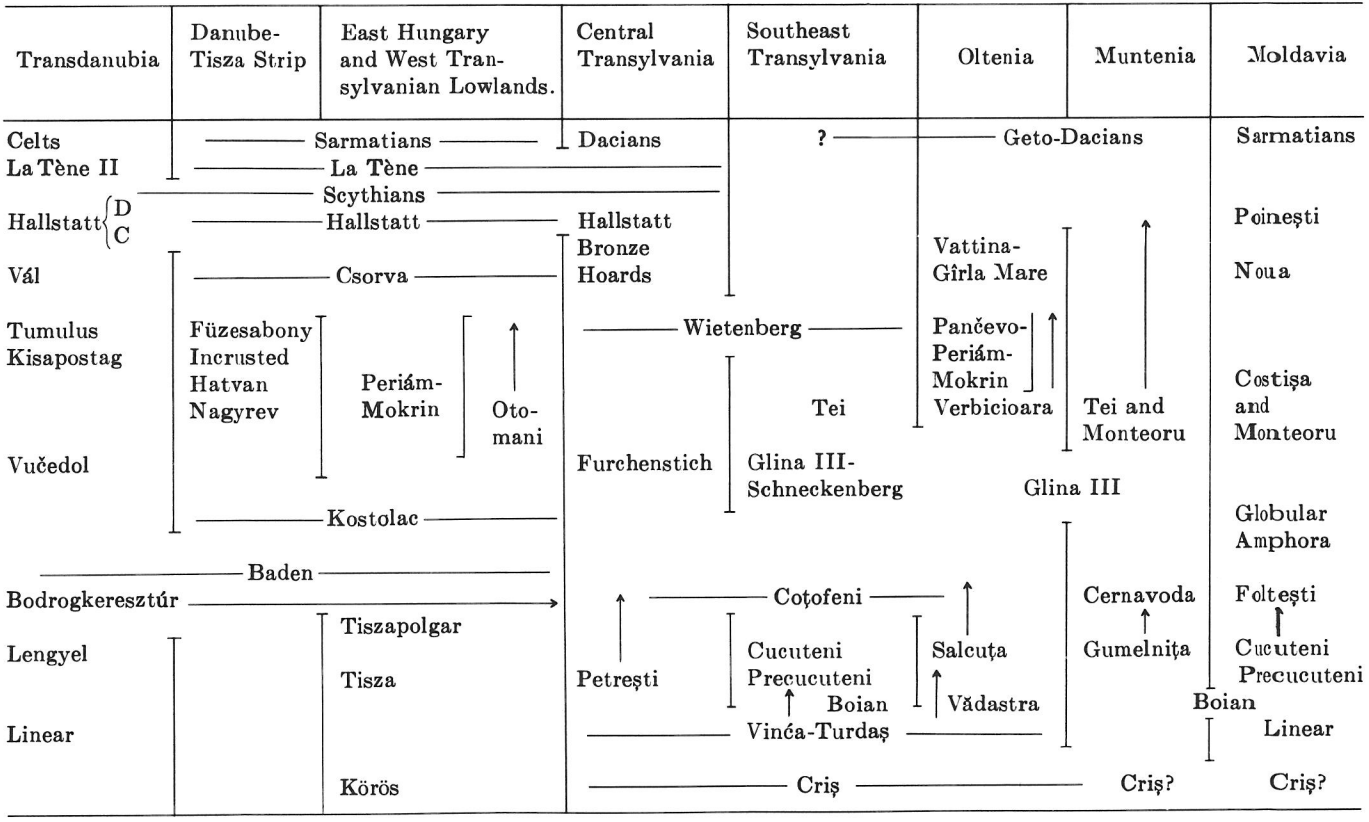
Northern Tier: Rough Table of Sequences in Major Areas



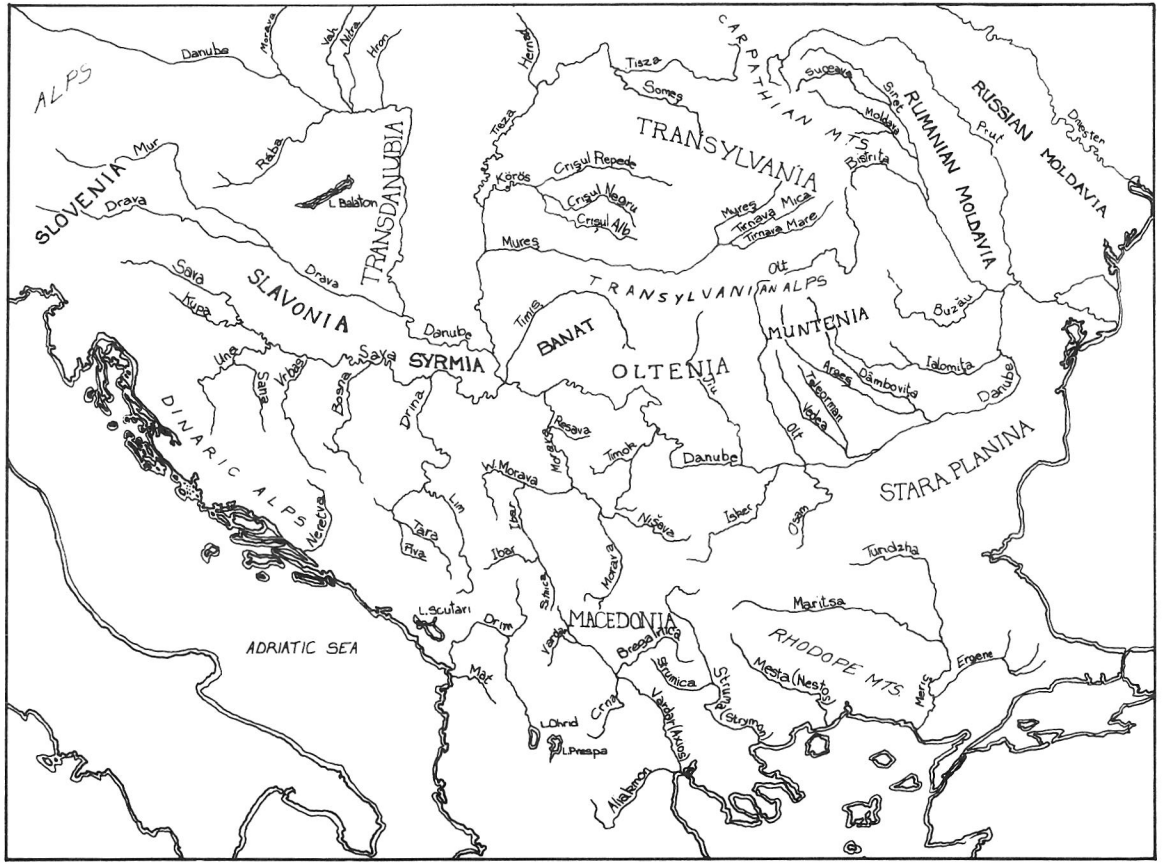


FIG. 1. General orientation map.

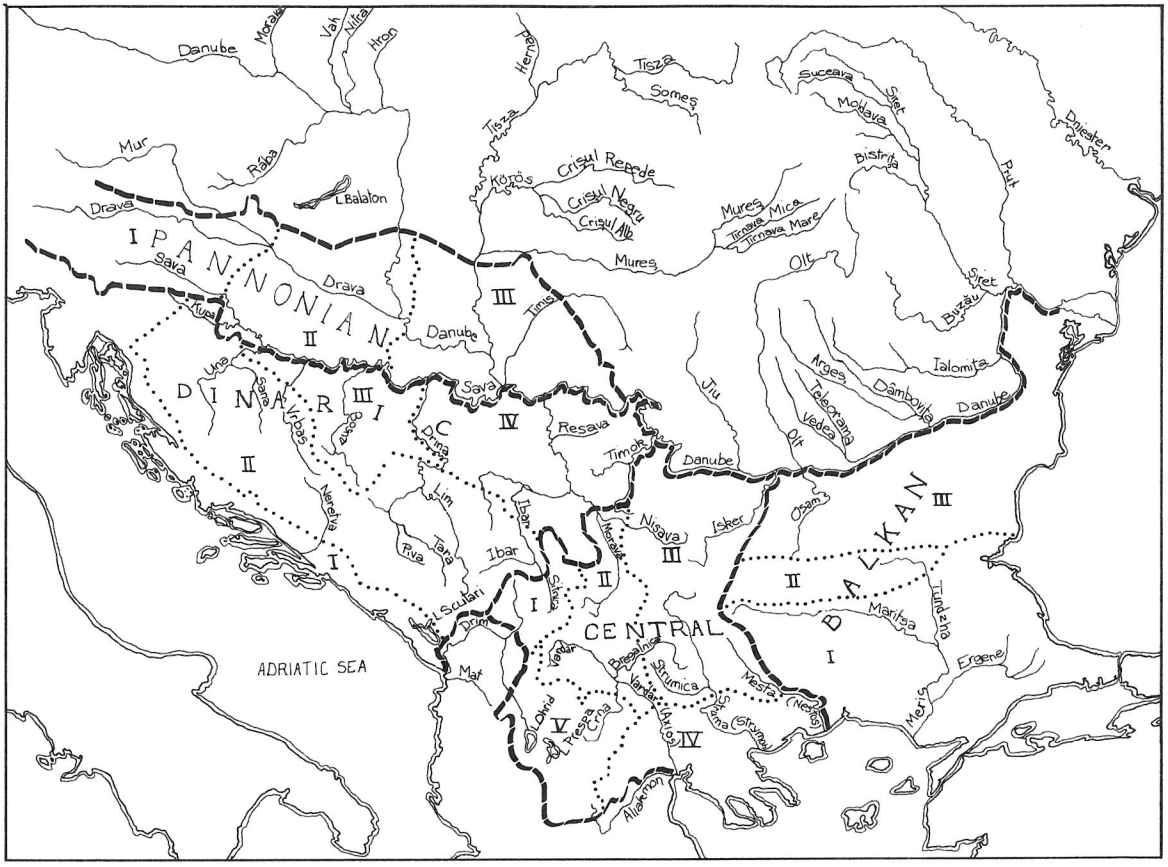
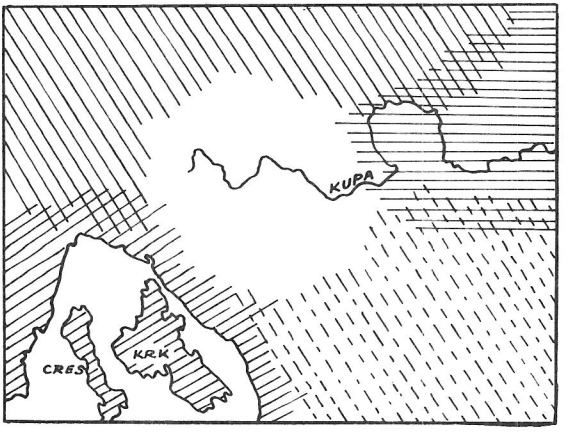


FIG. 2. Modern cultural ('psychic') types in the Southern Tier (after Cvijić 1918). *Pannonian*: I Alpine. II Slavonian. III Syrmian-Banat. *Dinaric*: I Dalmatian. II Eran. III Bosnian. IV Sumadijan. *Central*: I Kosovo-Metohijan. II Morava-Vardar. III Šopi or Torlaci. IV South Macedonian. V West Macedonian. *Balkan*: I Rumelian-Thracian. II Sredna Gora. III Lower Danubian.

FIG. 3. Modern culture regions: Southern Tier. (after Gavazzi 1956:11).



Fringe of the Alpine culture area
Fringe of the Adriatic culture area
Fringe of the Pannonian culture area
Fringe of the Dinaric culture area
Nodal area of strongest intermixture

FIG. 4. Nodal point in the northwest Balkans (after Gavazzi 1963:10).

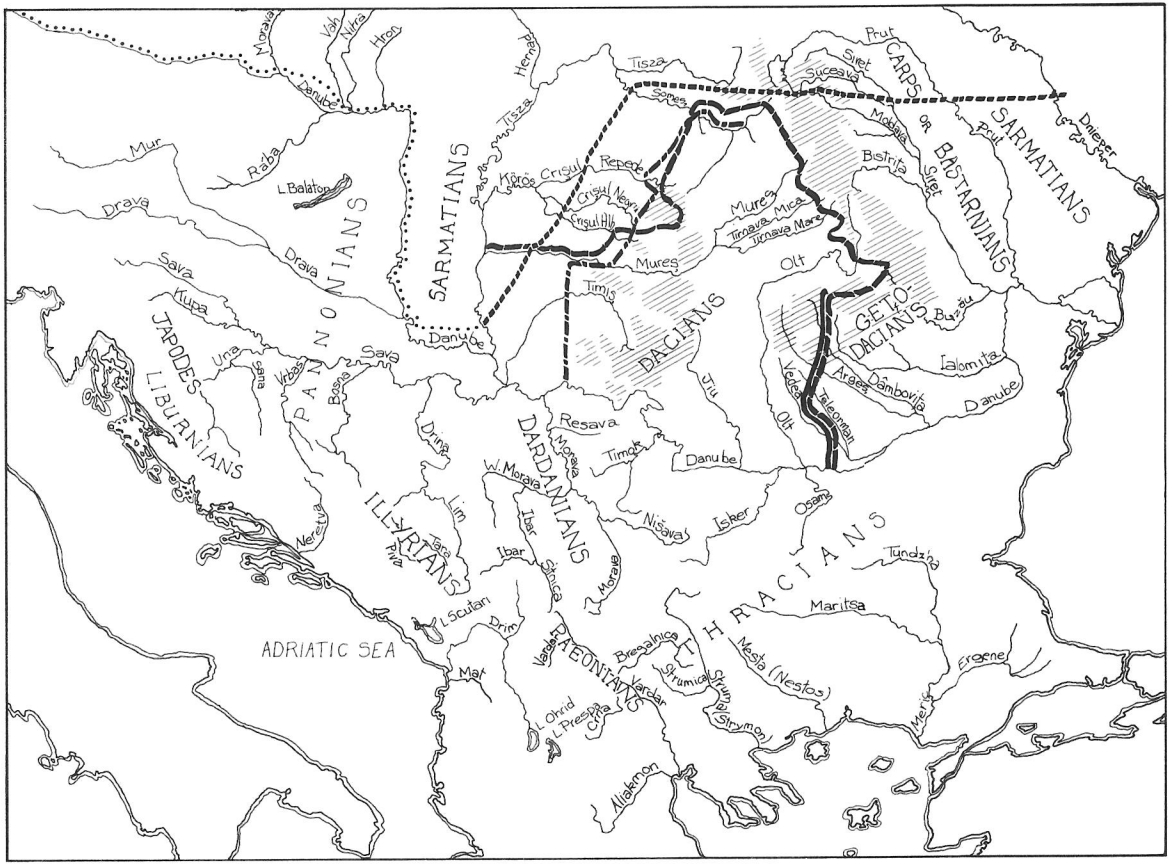
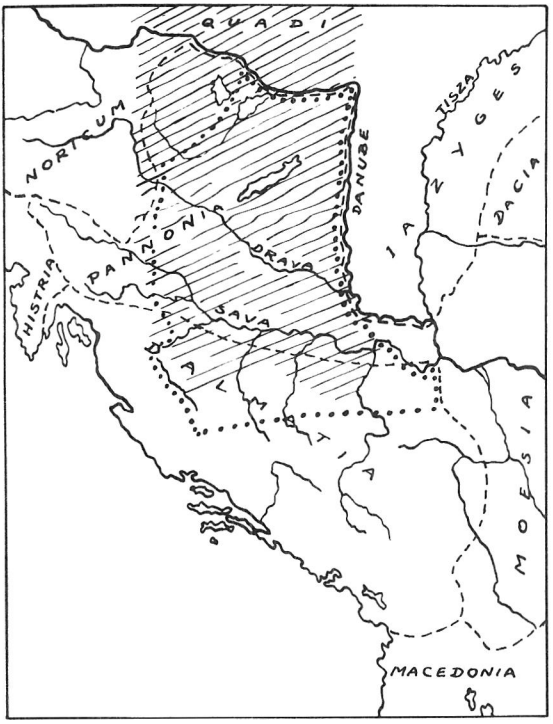
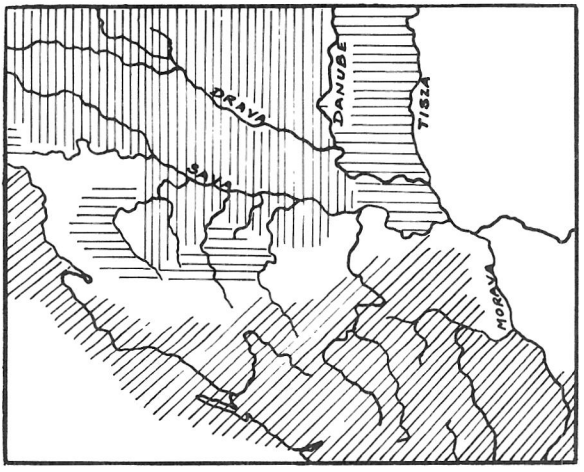


FIG. 5. Roman period: distribution of ethnic groups. • • • • • • • *Limes* and Imperial boundary (after Kiepert and others) - - • - - • West Dacian *limes* (after Szilagyi 1943: 91) ——— *Limes Transalutanus* (after R. Vulpe 1961:374) — — — Dacian boundary (after Protase 1962:175).



Pannonian cultural complex
..... Approximate habitat according to Strabo
----- Boundaries of the Roman provinces

FIG. 6. Pannonian distribution: archaeological and according to Strabo (after Marić 1964, Prilog 2; following p. 213).



Urnfield distribution—Pannonian
Ceramic finds related to Urnfield cultures
Tumulus distribution—Illyrian

FIG. 7. Distributions suggesting Pannonian and Illyrian territories (after Marić 1964: following p. 213).

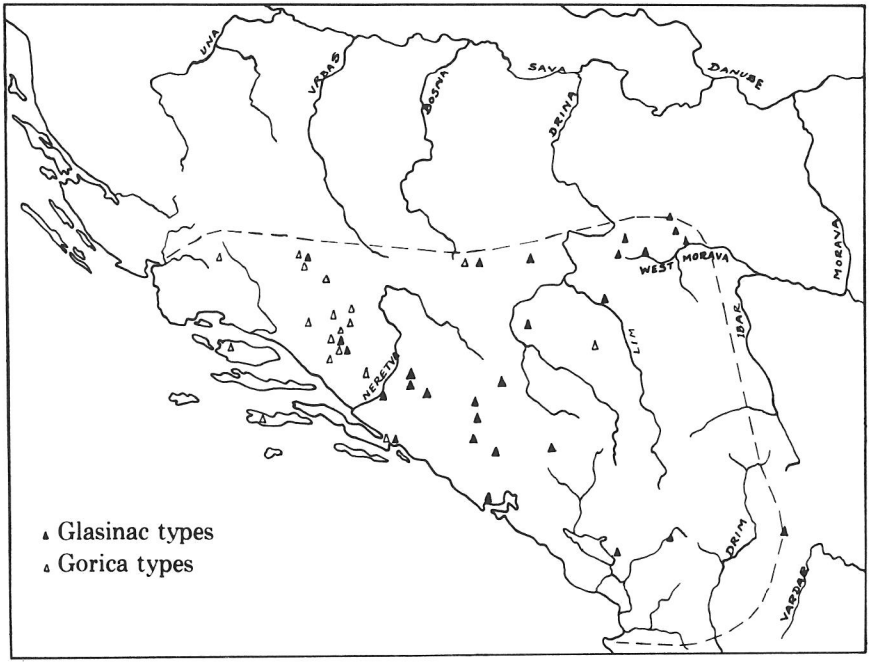


FIG. 8. Distribution of Gorica and Glasinac types in Central Illyrian Area (after Čović 1964: Map IV, following p. 134).

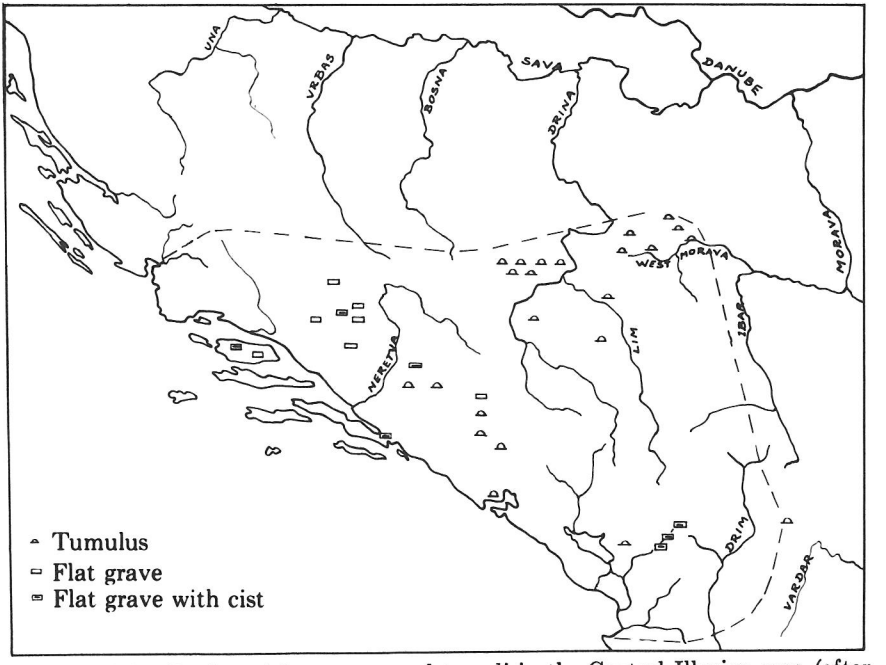
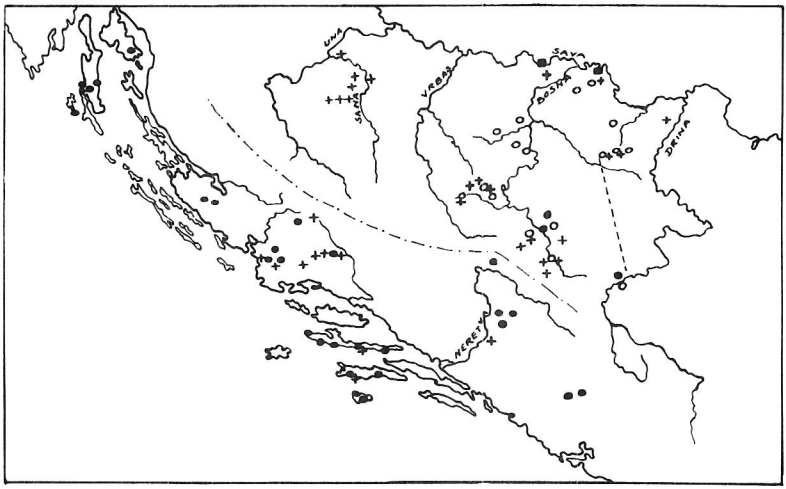


FIG. 9. Distribution of flat graves and tumuli in the Central Illyrian area (after Čović 1964: Map II, following p. 134).



∘ Neolithic sites of inland cultures
• Neolithic sites of coastal cultures
+ Eneolithic sites ▪ Slavonian-Syrmian outliers
–·–Approximate line of Adriatic watershed
--- West Boundary of Starčevo and Vinča cultures, after Benac 1959, p. 9. (Also eastern extent of Vučedol culture.)

FIG. 10. Neolithic and eneolithic site distributions in Western Yugoslavia (generalized after Benac 1962: Figs. 3 and 7).

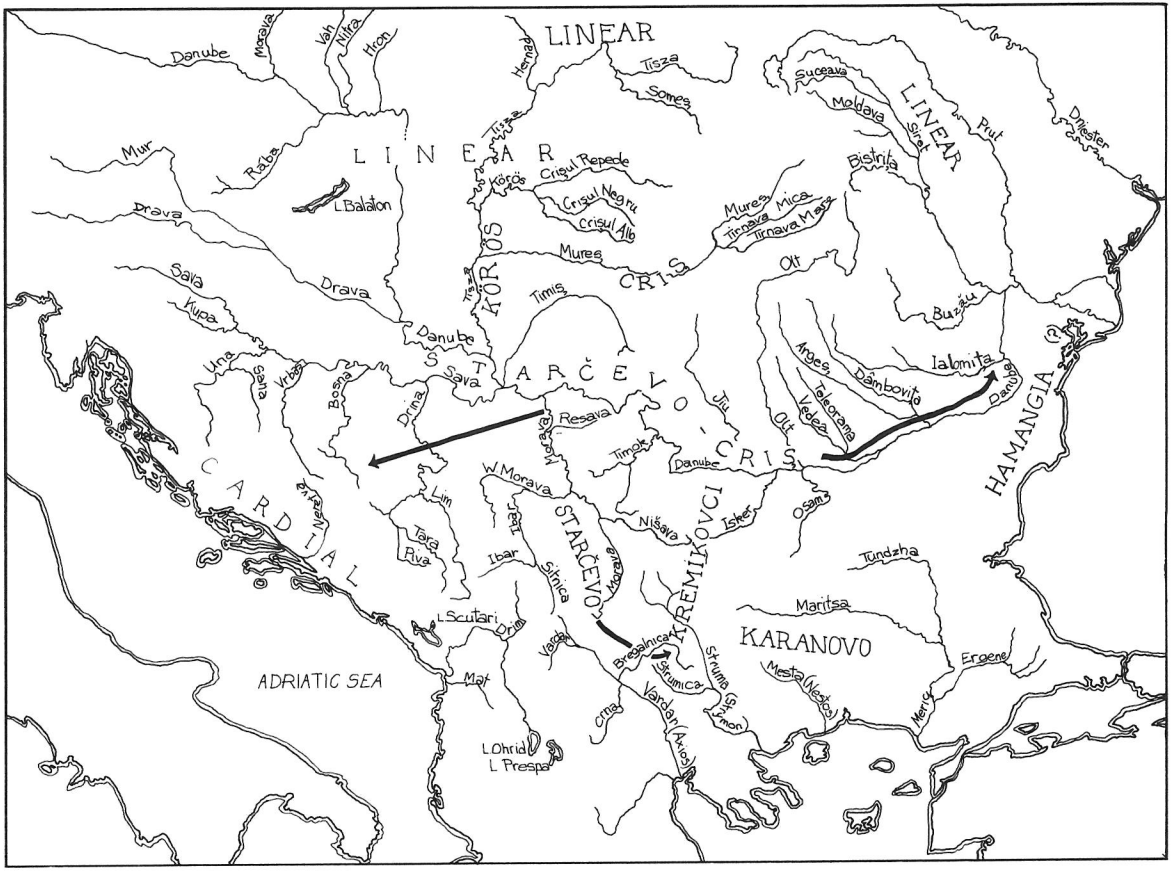


FIG. 11. Early Neolithic: generalized distributions.

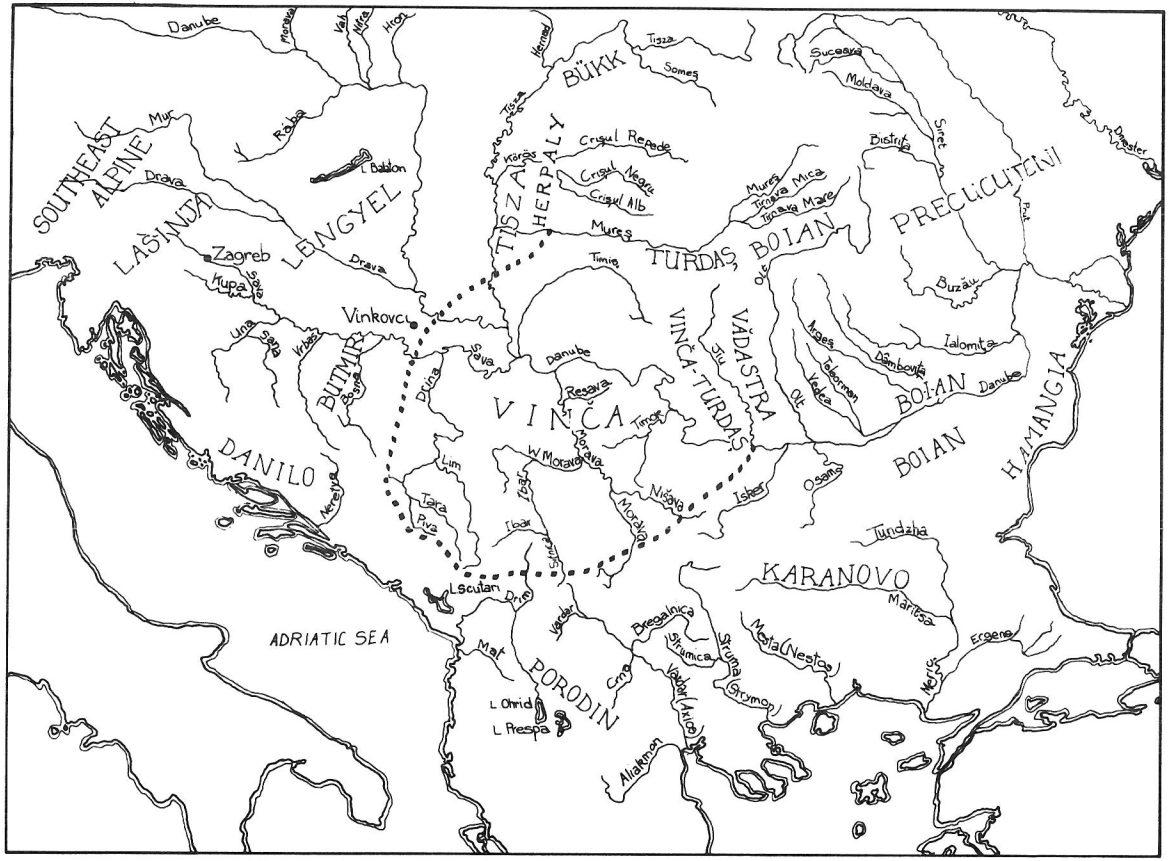


FIG. 12. Middle Neolithic: generalized distributions.

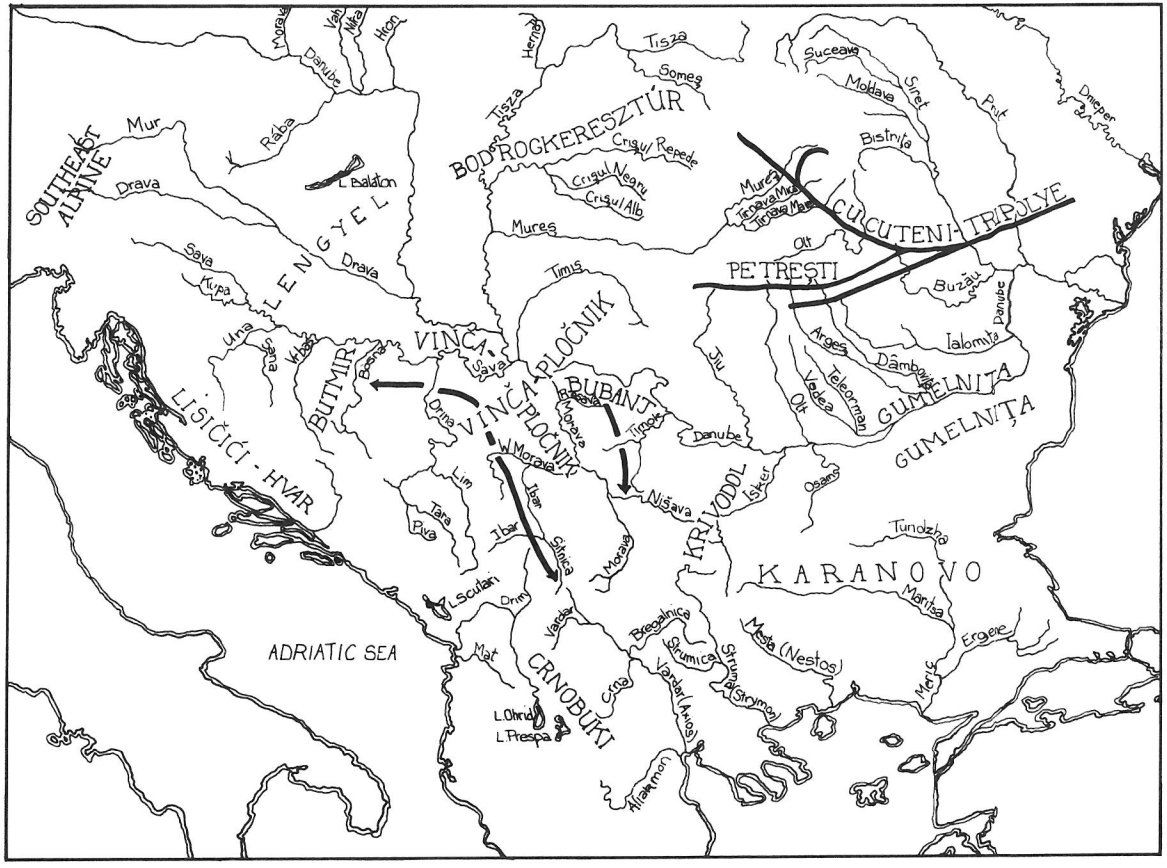


FIG. 13. Late Neolithic: generalized distributions. (Boundary after Dumitrescu 1963:304).

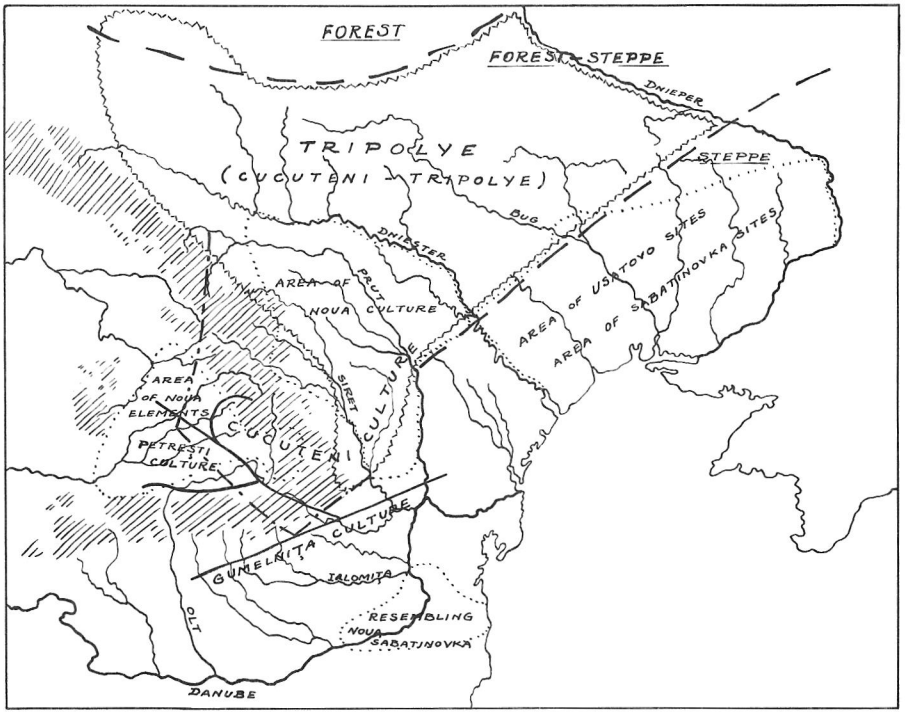


FIG. 14. Generalized distributions. Cucuteni-Tripolye and Usatovo. ——— after Dumitrescu (1963:304) ≈≈ after Gimbutas (1956:101) - - - partly after Roman (1963:34) and R. Vulpe (1956:Fig. 1) (Noua and Sabatinovka): ····· after Florescu (1964:190).

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The Evolution of Pastoralism and Indo-European Origins

Ward H. Goodenough

Method in prehistoric study rests first on how acceptable our inferences are, as judged by their compatibility with all forms of reliable evidence, and second on how plausible they are, as judged by existing knowledge of social and other processes, induced and verified in empirical study. Reliable evidence is never complete and usually allows of several acceptable but mutually contradictory inferences. Knowledge of how the phenomena in question generally behave is rarely what we would like it to be. Nevertheless, both the quantity and variety of evidence continually grow, as does our scientific sophistication, so that prehistoric problems become increasingly capable of resolution as we reexamine them in the light of new information and expanded intellectual resources. The origin and spread of Indo-European speaking peoples is, in this respect, an archetypical problem in prehistory.

Definition of this problem derives, of course, from inferences drawn from linguistic study. If we were not assured of the existence of a family of genetically related languages that by all the canons of plausibility must be derived in significant part by one or another route of transmission from a common source language, there would be no Indo-European problem. Moreover, empirically based knowledge of linguistic process makes highly implausible any definition of the problem that does not assume that the source language consisted of a group of mutually intelligible, geographically contiguous dialects, whose distribution, given existing modes of travel and communication, permitted the kind of social interaction without which no such speech community or *Sprachgemeinschaft* has been known to exist.

By the beginning of recorded history, the Indo-European languages had a distribution that extended far beyond such boundaries and were already differentiated into a series of mutually unintelligible languages and groups of languages. To describe the routes by which the spread took place, to locate the region from which it started, and to explain why and how it took place at all, these are the tasks that define the Indo-European problem.

Internal comparison of Indo-European languages has produced the first body of important evidence. By establishing regularities of phonological correspondence in semantically similar words, it has been possible to con-

struct a reasonable facsimile of the phonological patterning of the common Indo-European language and to compile a partial lexicon of forms for that language with their approximate meanings. This provides evidence of the subject matter of discourse in the source language, and this subject matter in turn is evidence of the way of life of its speakers and of the geographical region in which they lived.

From such linguistic data it has been argued that the Indo-European homeland was either in central Europe or the South Russian steppe. Proponents of the former position based it on the common vocabulary for flora and fauna that could be reconstructed for the source language. Proponents of the steppe homeland argued that the crucial flora and fauna terms are known only from the European languages in the Indo-European family and might have come into use when Indo-European peoples first moved from the steppe westward before their dialects were sufficiently differentiated for these innovations to be distinguishable in their phonological patterning from the vocabulary of the source language. Since the linguistic evidence indicated that the original Indo-European speakers put more emphasis on animal husbandry than on agriculture, they argued that it was reasonable to assume a steppe origin, because the steppe was the presumed natural habitat of pastoral peoples. The association of chauvinism and racism with the European position helped predispose some scholars to favor or oppose it on ideological grounds, so that the issue became burdened with emotional freight. More recently, Thieme (1954) introduced linguistic evidence from the Indo-Iranian languages to show that these also contain reflexes of the vocabulary that indicate a north Central European origin, specifically the North European Plain of Germany and Poland. His thesis has been critically reviewed by Krogmann (1955–56, 1960), who also concludes, however, that the evidence requires the Indo-Europeans to have come from a region west of the beech tree line (from the Baltic to the Crimea) in which salmon rivers also occur.[1]

The second major body of evidence is archaeological. Its application to the Indo-European problem requires that the various archaeological assemblages be dated relative to one another, that the various technological and stylistic traditions be sorted out and their interconnections and derivations traced, and that specific assemblages be grouped according to similarities and dissimilarities into cultures and cultural traditions that can be presumed to have some correspondence with ethnic groups. The crucial thing, of course, is to link languages with cultural traditions.

As Childe (1926) observed in his review of the archaeological issues, only one among the several cultural traditions of Europe has a temporal and spatial distribution that approximates the distribution Indo-European languages must have had shortly before their speakers began to penetrate the Mediterranean and Middle Eastern lands. This tradition is represented by a series of cultures distributed, in the second half of the third millennium B.C., from the Rhineland across the North European Plain and the Russian steppes all the way to the Altai Mountains. Best known from

graves, these cultures are variously referred to as Battle-Axe, Corded Ware, Separate grave, Tumulus, or Kurgan cultures. Traces of the corded ware associated with them appear in assemblages to the south of this area, but the spread of early Indo-European cultures in the Aegean and Middle East is most directly connected with other ceramic traditions, such as the Minyan wares of northwest Anatolia and Greece and the gray wares in Iran (Hissar II) and a narrow belt extending eastward into Afghanistan. Indo-Europeans entering the southern areas of richer civilization took over the products of local technology, making use of local craftsmen trained in more sophisticated techniques.

When Childe wrote, dating was too crude to provide any archaeological evidence regarding the direction of movement of the Battle-Axe cultures. Critical of the Scandinavian hypothesis on good linguistic and archaeological grounds, frankly repelled by the racism of some advocates of a north European origin, Childe accepted what seemed the more authoritative linguistic judgment of the time, as put forth by Schrader (1890), and interpreted the archaeology to fit an origin in the South Russian steppe, reversing the thesis of most continental authorities of his day regarding the direction of migration of Battle-Axe cultures. His has since become the accepted position of the foremost British and American synthesizers of European prehistory (Piggott 1965; Gimbutas 1963, 1965).[2] Gimbutas (1963, 1965), following most recent Russian work, has departed from Childe to the extent of deriving the Kurgan cultures from the steppes on the Lower Volga and farther east, rather than from the Kuban, where Childe (1929:193) felt they might have started. While linguistic opinion has been moving in the direction of putting the Indo-European homeland in the region of the Vistula, Oder, and Elbe, archaeological opinion is now putting it in the Lower Volga steppe and regions east of the Caspian Sea.

Mixed up in the current archaeological position, especially as it involves excavations and their interpretations by Russian archaeologists, is an implicit assumption about the evolution of pastoralism. It is clear from the linguistic evidence that early Indo-European societies, although acquainted with grain agriculture, were much concerned with animal husbandry. The Battle-Axe cultures show a definite pastoral orientation; and a well developed pastoralism would help account for the tremendous range of their dispersal. Pastoralism is most highly developed as a distinctive way of life in the Eurasiatic steppes. Presumably, then, this is where it originated.[3] Childe (1958:149) is explicit on this point, suggesting as one possibility that Mesolithic hunters in the river basins and on the Black Sea coasts may have been 'converted by their neighbour's example to food production', subsequently evolving the distinctive pastoralism that has ever since been associated with steppe peoples. Indeed, the entire steppe hypothesis hinges on this point, for the Kurgan (Battle-Axe) cultures are the oldest food-producing cultures to appear in the steppe.

Pastoral nomadism in the steppes achieved its classic form with the Scythians, whose horseback riding revolutionized steppe warfare. But it

was not significant for warfare alone. With the development of breeds of horses capable of supporting a man's weight and the exploitation of this development, it became possible to assemble and move larger herds over greater distances and to protect them effectively. The scope of herding operations in the steppe lands seems greatly to have expanded during the Scythian period. It was at this time, among other things, that the steppes east of the Altai Mountains were first penetrated by pastoral nomads. Before horseback riding, speedy transportation depended on using a team of yoked horses to pull a light car—a chariot. While far less efficient than horseback riding, this invention, associated with Indo-European pastoralists, must in its day have served significantly to increase the scope of pastoral activity, facilitating the round-up of herds on open ranges and their movement from one range to another. Before the chariot, pastoralism in the steppe must have been restricted to operations that could be handled by men moving on foot, aided by dogs. This would have imposed limitations on the kinds of animals kept and on the manner and numbers in which they were kept. It would have confined herding operations to the most favorable areas: at the forest edges, in the park lands, and close to the rivers, whose basins were also forested. It is not surprising that it is precisely from such areas that the early Kurgan sites of Russia are reported (Gimbutas 1956, 1963a, 1965). Early pastoralism, it seems, was far from being well adapted to the Eurasiatic steppes and was capable of exploiting them only to a limited degree. Indeed, Gimbutas (1965:482) describes the Kurgan I and II peoples as 'basically small-scale stock breeders, hunters, and farmers', apparently following a mixed economy rather than a full-fledged pastoral nomadism.

In these respects the early Kurgan cultures resembled the neolithic North Pontic or Mariupol Culture that was developed by late mesolithic inhabitants of the Dnieper and Donets river basins. They were evidently not the people from whom the early Kurgan culture arose, but the Kurgan I and II pottery shares some features with North Pontic pottery, and there seems eventually to have been a blending of the two populations in South Russia, as suggested by finds at Aleksandrija (Gimbutas 1965:480–481). Gimbutas (1963a, 1965) is definite that the Kurgan cultures are intrusive into the North Pontic area. It is not unlikely, indeed, that the North Pontic culture developed in response to influences from Kurgan people.

If the earliest pastoralists in the south Russian steppe did not stem from the mesolithic population that occupied its river basins, but entered the region from elsewhere, then where did they enter it from? Gimbutas (1963a, 1965) holds that they must have entered from steppe lands farther to the east. This implies that somehow the mesolithic people of this more eastern area, stimulated by remoter contact with civilizations to the south, must have done what the mesolithic people of South Russia evidently failed to do; or it means that people with a neolithic economy moved into the eastern steppes from the south, bringing a pastoral pattern with them. So far there is no firm evidence to support either position. Furthermore, the southern

steppes east of the Caspian Sea are arid and present an effective barrier to any penetration by agricultural people from the south, except where irrigation is possible. The path of migration from northern Iran and Afghanistan leads eastward into Kashgaria, not northward into the steppe. The extreme lateness of steppe nomadism east of the Altai, moreover, argues firmly against its early development from an eastern source.

This leaves only two other areas from which early pastoralists might have penetrated the steppes: the western Ukraine and the Caucasus. Gimbutas (1965) observes that the earliest Kurgan cultures in the Kuban area, exemplified by the Nal'chik burials, are of Kurgan II–III type. The early pastoralists, it appears, were already situated in the vicinity of the Tripolye and North Pontic villages to the north and west before they came into the Kuban. This appears to eliminate the Caucasus as a contender. It leaves the western Ukraine as the region from which early farmer-herdsmen began to penetrate the steppe.

Hardly any of the early Kurgan sites are firmly dated. Gimbutas (1965: 478) reports a carbon-14 date of 2994 ± 200 B.C. for the earliest pottery at the locality of Strelka in the stratified site of Gorbunovo on the eastern side of the Middle Urals. This pottery is subneolithic, relating the assemblage of materials from this earliest Strelka level to pre-Kurgan peoples (Gimbutas, 1958). The accompanying materials, entirely in keeping with a fishing and hunting population, show no evidence of animal husbandry. This date, moreover, is much later than the carbon-14 dates for Pre-Cucuteni and Cucuteni A in Moldavia and the Western Ukraine; and Kurgan I remains in the Dnieper area show clear evidence of contemporaneity with Pre-Cucuteni III (Tripolye A), according to Gimbutas (1965:478–9). This could put the Kurgan I culture in the western Ukraine as far back as 3700 B.C., judging from carbon-14 dates for Cucuteni and other cultures in the eastern Balkans (Ehrich, 1965:440–1). These dates can, of course, be interpreted as evidence that Kurgan I people moved into the Western Ukraine from the east at an early date and that when more archaeology is done in the east the evidence of their early presence there will eventually be forthcoming, in spite of the lateness of the sub-Neolithic at Strelka. This, if I read her correctly, is the position taken by Gimbutas (1965). Such a position can be supported at present, however, only by virtue of its reasonableness in the light of assumptions about the origins of pastoralism.

Closely linked with these assumptions is the assumption that the horse must have been first domesticated in the Eurasiatic steppe. The earliest known horse bones in a neolithic context come from an early Tripolye (Pre-Cucuteni III) site in Moldavia, and from the lowest stratum of the Kurgan site of Mikhajlovka, west of the Dnieper on the southeastern border of the Tripolye area. But the unfounded assumption of eastern domestication leads Gimbutas (1965:479) to comment: 'That the horse was brought to Europe by the Kurgan people from the east is more likely than that its breeding was independently started by the Tripolyans in the upper Dniester Valley. The presence of horses is well evidenced in all other Kurgan periods.'

Clearly the distribution of the horse in all other, i.e. later, Kurgan periods can be relevant only if one assumes an eastern origin of the Kurgan culture.

I can only conclude that at present, the evidence is more consistent with the idea that the Kurgan cultures moved into the steppe from the west. Whether the western Ukraine is to be interpreted as the home of the entire Battle-Axe complex of cultures and of the Indo-European source community is another problem. Certain it is that for a long time, as Gimbutas (1965) observes, Kurgan I and II peoples were living in close association with Cucuteni and Tripolye farmers.[4] Their movement eastward into the steppe appears to have been into the region of the North Pontic peoples first. From there, eastward movement continued to be limited to the forest edges along the rivers and by the Ural mountains. Only later did they venture more widely into the open steppe lands, as they developed the light, horse-drawn car or chariot and then breeds of horses that could carry mounted men. These latter developments undoubtedly contributed to subsequent movements back into Europe just as they contributed to movements into the Middle East. Once the steppe was conquered, it inevitably became a source of out-migration. Until it was conquered, it was a waste, waiting for people on its periphery to evolve a technology that would allow them to begin to move in and exploit it. This brings us back again to the evolution of pastoralism.

Pastoral nomads are almost everywhere dependent on agriculture. Either they must do some planting themselves or they must trade with farming communities. Pure pastoralists, as distinct from people with mixed farming and animal husbandry, are rarely able to be self-sufficient.[5] The dependence of pastoralists on farmers in antiquity is illustrated by Herodotus in his account of the Scythians. Pastoral nomadism is, moreover, a highly specialized mode of life. As Krader (1955) and Barth (1959, 1961) observe, it involves regular patterns of movement over a series of pasturages suitable for different seasonal conditions and requiring clearly understood allocations of grazing and water rights at different times among different groups, which move over well-defined routes. Problems of traffic control are often complicated and may require central political authority to regulate (Barth, 1961).[6]

Much more widespread and as much as several thousand years older is mixed farming and herding. It often takes the form of transhumance, in which 'the livestock not being used for food or work are driven out to the pastures seasonally by village herdsmen while the core of the village remains at home and tills the soil' (Krader, 1955:302). The specialized nature of nomadic pastoralism and its continued symbiotic dependence on trade associations with agricultural communities make it highly unlikely that it could have arisen except as a more specialized outgrowth of an earlier transhumance.

The neolithic economy, with mixed agriculture and animal husbandry, is now known to have come into eastern Europe from Anatolia as early as 6000 B.C. (Weinberg, 1965a, 1965b).[7] By 5000 B.C. sites of the Starčevo-

Körös culture were distributed in northern Greece, Bulgaria, Rumania, the Banat, Hungary, Transylvania, and Moldavia (Piggott, 1965:43). By 4500 B.C. a special adaptation of this economy to the mixed-oak forest of central and northern Europe had been made by bearers of the Danubian culture, whose characteristic 'linear pottery' (Bandkeramik) is distributed through Czechoslovakia, Austria, the Rhineland, Saxo-Thuringia, the Upper Elbe, the Vistula, Ruthenia, and Moldavia (Piggott, 1965:51; Thomas, 1965a). At the same time, neolithic communities had become established in southern France, from which they subsequently spread northward into France and Britain. The mesolithic hunting people of northern and western Europe took up parts of the neolithic economy, as indicated by the Campignian culture of northern France and the Ellerbek-Ertebølle culture of Schleswig-Holstein (Thomas, 1965a, 1965b). By 3400 B.C. there had emerged a series of related cultures in northern Europe characterized by an emphasis on animal husbandry, a preference for beaker-shaped pottery, and burials under barrows. Thomas (1965a:352) associates the emphasis on cattle-keeping, so characteristic of these cultures, with the shift from the moister Atlantic to the drier sub-Boreal climate, which presumably led to the production of more open country, especially in the highlands. There is experimental evidence, moreover, to indicate that systematic burning of felled forest over an area to be planted was necessary in northern Europe in order to change the acidity of the soil so as to permit grain to grow (Iversen, 1956). After harvest, such land went into perennial grass rather than reverting to forest.

Sod, it appears, was not readily tillable until the development of the moldboard plow in the Christian era. To keep good agricultural land in production, therefore, it was necessary to pasture stock on the stubble after harvest for manuring purposes. Land that had been fallow for a year or two could be tilled once more, especially on the lighter soils, but to achieve intensive enough use required running animals on it. These same animals, however, had to be pastured elsewhere during the growing season. Thus the requirements of neolithic agriculture in central and northern Europe seem to have implemented an expansion of herding activity considerably beyond the modest scale on which it was practiced by the early Danubian farmers. The development of transhumance was a natural response to the ecology of primitive farming in neolithic Europe.

By 3000 B.C. it is clear that mixed farming with strong emphasis on herding was already well established. It must have involved transhumance and styles of living appropriate thereto. The cultures distributed across the North European plain at this time belong to what is called the Funnel Beaker group, whose sites extend from Scandinavia to Poland and south to the Carpathians and central Germany. Contemporary with them were the Michelsberg sites in the Rhineland. Also contemporary with them, judging from their association with Pre-Cucuteni III and Tripolye A, were the Kurgan I and II sites of the Western Ukraine.

As Malmer (1962) has observed, the corded-ware beakers of the Battle-

Axe cultures can be interpreted as regional variations within a European group of Beaker cultures that includes the funnel beakers of northern Europe and the tulip beakers of the Michelsberg culture.[8] Their contemporaneity over a continuous territorial tract lends support to this view. The presence of stone battle-axes and stone-encircled graves surmounted by barrows among the Funnel Beaker cultures as well as among the Kurgan cultures, is further witness to the extent to which these cultures were drawing on common traditions and common sources of outside influence.

The Battle-Axe cultures were carried westward in the Kurgan III stage (Gimbutas, 1965) around 2700–2600 B.C., judging from carbon-14 dates in western Europe (Thomas, 1965a); and over the next several hundred years they replaced the Funnel Beaker cultures. But this movement does not contradict the earlier contemporaneity of Battle-Axe and Funnel Beaker cultures on a geographic continuum across northern Europe. The reasons for this spread and the manner in which it took place remain obscure. Malmer (1962) observes that the arrival of Battle-Axe cultures in Scandinavia was associated with a shift of emphasis in herding from cattle and pigs to sheep. He concludes from his analysis that there was great carryover from the previous Funnel Beaker cultures, the overall change from Funnel Beaker to Battle-Axe cultures being no greater than the earlier changes from one stage of Funnel Beaker culture to another. This may be an overstatement. Nevertheless, the infiltration of Battle-Axe peoples into northern Europe was evidently followed not by the annihilation or displacement of Funnel Beaker people, but by fusion and amalgamation with them, as Gimbutas (1963b:49) indicates took place in the East Baltic region. Instructive as to how this might have occurred is Barth's account (1964a) of the expansion of Baluchi-speaking Marri at the expense of Pashto-speaking Pathans in northeast Baluchistan by a process of political incorporation and linguistic absorption facilitated by differences in the tribal organization of the two peoples. But here I am going beyond the scope of this paper.

What I have been saying obviously leads to a conclusion that the Battle-Axe or Kurgan cultures originated about 3500 B.C. in an area bordering on the region occupied by Cucuteni and Tripolye peoples, who themselves practiced a mixed farming and herding economy. The latter cultures were at that time and for another thousand years part of a larger cultural province that extended southward to include the Salcuţa and Turdaş (Tordos) cultures of Transylvania, the Gumelniţa culture of the Dobruja, the Karanovo culture of Bulgaria, the Dimini culture of Thessaly (Childe, 1929:107), and Late Calcolithic 3 and Early Bronze I cultures of Thrace, the Troad, Lemnos, and Lesbos (Mellaart, 1962). Western Anatolia was the center of a vigorous metallurgical industry that exported all over this region and beyond it to Central Europe. It strongly influenced the copper industry that began to develop in Transylvania during this same period. The megaron house forms of Troy I (Blegen, 1963; Mellaart, 1959) and earlier are matched by wooden frame houses of megaron plan at Ariuşd in Transylvania (Childe, 1929:99). The cemeteries at Yortan and Kusura

in Northwest Anatolia (Mellaart, 1962:16) contained single grave burials in cists or in *pithoi* in pits topped with stone slabs. The bodies were in tightly flexed position and the accompanying grave goods included weapons in the form of daggers or axes. Polished stone battle-axes are reported from Troy, Thermi, Yortan, and Alişar for this Early Bronze I period (Mellaart, 1962:18). Similar burial patterns, with battle-axes and other weapons among the grave goods in "royal" tombs, continue in western Anatolia in the Early Bronze II period from 2700–2300 B.C.[9] (Mellaart, 1962:30–1). An elongated copper hammer-axe is reported for the Turdaş culture of Transylvania (Childe, 1929:75–6) and another copper hammer-axe from the Lucka cemetery in Ruthenia (Childe, 1929:86, fig. 56). Furthermore, generally similar burial customs are reflected in widely distributed cemeteries in Central Europe in association with the Lengyel and Tisza cultures (Childe, 1929:88–89). The presence of battle-axes in western Anatolia in Early Bronze I times is not in itself evidence that there were Indo-European elements already represented in its population. But it is clear evidence that the concurrent popularity of battle-axes in northern Europe and later in the Eurasiatic steppes in predominantly single graves containing contracted burials (with or without circular mounds or stone-line cists) cannot be construed as unrelated to these Anatolian and east central European phenomena. Even a megaron house is reported for a Battle-Axe site on the Frisches Haff (Gimbutas, 1963b:51). The source of these traits must have been somewhere on the northern fringes of the West Anatolian and East Balkan cultural province.[10]

What I have been saying applies most specifically to the problem of the origin of the Kurgan or Battle-Axe cultures, which I have portrayed as a local, east European culture that was linked with the Funnel Beaker cultures to the west in a number of traits and at the same time linked to the East Balkan-West Anatolian cultural province in a number of others. But is the homeland of the Kurgan cultures to be equated with the homeland of the Indo-European speech community? Certain it is that much of the spread of Indo-European languages is directly associated with the Kurgan-Battle-Axe cultures, but this does not mean that only the Kurgan I people were Proto-Indo-Europeans. It is a reasonable inference, to be sure, if one sees the Kurgan I people as nomads moving into Europe out of the Lower Volga and regions farther east. But if one sees them as I have presented them here, such an inference is no longer compelling. It means that Proto-Indo-European was a European language among other European languages. It is quite possible that closely related languages and dialects were spoken across the entire north European plain by Funnel Beaker and Kurgan I peoples, that the Kurgan I people were not THE Proto-Indo-Europeans, but a subgroup among them.[11] On the other hand, the Kurgan I people may have spoken a language that was related to the languages spoken by peoples in the East Balkan-West Anatolian cultural province to the south.

As the reconstruction of Proto- and Pre-Indo-European has progressed, it has become increasingly evident that the earliest stage may have had a

phonological structure of a peculiar sort in that it lacked distinctive vowels (Lehmann, 1952), a structure that is paralleled only by that of the Caucasic languages (Allen, 1956:172–4; Kuipers, 1960:104–5). Friedrich (1966) has cited this as evidence in favor of a north Caucasus origin of the Indo-European people. But here we are dealing with structural similarities that suggest a possible connection at a much greater time depth than is implied by the term 'language family'. It is possible that the Caucasic languages are survivors of a group of languages that were once widely spoken in Anatolia. Anatolia, it is becoming increasingly clear, was the region from which the neolithic economy was introduced into Europe by immigrant farmers who carried an Anatolian culture with them. Indeed, E. and J. Neustupný (1961:57–8) would trace the cultural pedigree of Proto-Indo-Europeans via the Funnel Beaker cultures back to the Lengyel culture of Czechoslovakia and Hungary, which goes back to the Vinča and earlier Starčevo-Körös of the east Balkan neolithic immigrants. The Cucuteni and Tripolye cultures also developed from this same source. From this viewpoint, the ultimate connection of Lehman's (1952:112) earliest stage of Pre-Indo-European with Caucasic, if indeed there was a connection, could go back to the neolithic colonization of Europe. But in such matters everything is wide open to all kinds of speculation.

The point remains that the archaeological evidence is entirely compatible with the linguistic evidence in pointing to the North European Plain, or at least its eastern end in Poland and the western Ukraine, as the most probable home of the Proto-Indo-Europeans. It is also compatible with a possibly much older linkage of Pre-Indo-European with the Caucasic languages.

The position I have taken differs from the one that has been espoused by Gimbutas (1963a, 1965), whose command of the relevant archaeological materials is impressive. I have relied heavily on her own reporting of the archaeology in coming to this different conclusion. In doing so, I have in effect done what Childe did (1926). He accepted the linguistic judgment of Schrader (1890) and looked at the archaeology to see if it would fit. His successors have continued, in effect, to do the same thing. Gimbutas has shown that the known archaeology can be interpreted to fit the Eurasiatic steppe hypothesis, provided other evidence points in the same direction. Since other evidence points elsewhere, I have undertaken this exercise to see if current archaeological evidence can be reasonably interpreted, also, to conform to the European hypothesis. I am satisfied that it can be, with less straining of the data; and I find that such interpretation has the added advantage of accounting for the evolutionary background of pastoral nomadism as an adaptation of an older European transhumance to the more difficult steppe environment.

Notes

[1] Krogmann (1960) showed that the distribution of salmon precluded an Asian homeland for Indo-Europeans but not a South Russian one. But he regards the beech tree evidence as incontrovertible. Drawing on Krogman's study (1955–56), Friedrich (1966) has pointed out that the Causasus Mountains also have beech trees. He disputes not the linguistic analysis, but the claim that only the North European plain

meets its resulting specifications. But if Gimbutas (1963a, 1965) is correct in her interpretation of the archaeology of the Kuban area, the cultures associated with Indo-Europeans seem to have come in there from the steppe rather than to have gone the other way.

[2] Many archaeologists on the European continent, on the other hand, have continued to favor a European origin of the Battle Axe cultures. Äyräpää (1933) takes this position in his survey of battle-axe cultures in Russia. More recently, Malmer (1962) has come to a similar conclusion in his monumental study of the Swedish and Norwegian Battle-Axe cultures. E. and J. Neustupny (1961) and Bosch-Gimpera (1960) have also espoused a European origin, but with different theories regarding its relation to the archaeological evidence.

[3] Thus Tretiakov and Mongoit (1961:14) speak of a direct transition from hunting and gathering to 'primitive stock-raising' in connection with the steppe area.

[4] From this point of view the accepted chronology of the several phases of Tripolye culture require re-examination. This chronology, based on the work of Passek (1949), puts the sites of Gorodsk type at the recent end of the sequence on typological grounds because they show corded ware materials. These sites lie in the forest zone just to the north of the classical Tripolye sites and may well have coexisted with them, as Kopytoff (ms) has observed in a critical review of Passek's work. See also the similar critique of Passek by Brjussow (1957: 270–4).

[5] The interdependence of pastoralists and farmers in southwest Iran and northeast Baluchistan has been described by Barth (1959, 1961, 1964a, 1964b). For a review of pastoralism in Africa, see Jacobs (1965), and for the ecology of pastoralism in central Asia, see Krader (1955).

[6] Barth's (1961) account of the political organization of the nomadic Basseri tribe in Iran and its former rulers' use of the nomads to control the lowland agricultural villages is very suggestive of how the rustic upland and more sophisticated lowland communities may have been welded together into petty Indo-European kingdoms in Central Europe and elsewhere in the Bronze Age.

[7] This date is given by Weinberg (1965b:16) for the coming to Greece of the earliest post-ceramic neolithic. A pre-ceramic neolithic that combined agriculture with extensive animal husbandry was even earlier (Weinberg, 1965b:13–14).

[8] That Michelsberg belongs to the central European rather than to the so-called Western tradition has been strongly argued by Arnal, Bailloud, and Raquet (1960).

[9] Mellaart (1962) puts the beginning of Early Bronze 2 (Troy II) at 2800 B.C. and Troy I correspondingly earlier. Mellink (1965) puts Troy II much later. Their disagreement hinges on how the similar pottery types in Troy II and Tarsus Early Bronze III are to be interpreted. Mellaart (1962, 1964) sees them as having been introduced into Cilicia (Tarsus) by Luwian invaders who spread all over western Anatolia from the Troad following the destruction of Troy II, which would already have been Luwian speaking or contained within its cultural province Luwian speaking groups. In this way the wares came into Tarsus following their termination at Troy, and Troy IIB is earlier than Tarsus EB III. Mellink (1965) sees these wares as exported to Troy from Tarsus in trade by way of an as yet undocumented sea route and therefore makes Troy II and Tarsus EB III contemporary. Mellaart's interpretation gives Troy I and II a better date in relation to the eneolithic cultures of Europe, whose dates are better fixed by carbon-14. Mellaart's dates have been accepted by Weinberg (1965a) as providing a more satisfactory basis for interpreting Aegean archaeology as a whole.

[10] Failure to include this province for discussion and evaluation as such in *Chronologies in Old World Archaeology* (COWA) detracts from what is otherwise an admirable volume.

[11] It is worth noting that the geographical area in which Krahe (1954) finds his concentration of Old European (*alteuropäisch*) place names corresponds exactly with the region where Funnel Beaker cultures were dominant. His Old European is a language very close to Proto-Indo-European showing some minor phonological shifts from it. This would accord either with a view that the Funnel Beaker and Kurgan peoples were both Indo-European or with a view that Old European represents a dialect of Indo-European that emerged following the Indo-Europeanization of Funnel Beaker

people in the course of their amalgamation with incoming Kurgan or Battle-Axe people.

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The Basque Language and the Indo-European Spread to the West

Antonio Tovar

1. Basque is the only language in western Europe preserved from the period prior to the IE invasions. Although lexico-statistics show some appreciable relationship to Hamitic and also probable connections with some western and southern Caucasian languages,[1] no real close similarity proving common origin has been discovered.

Likewise, its relation to Iberian, to the limited extent to which this language is known, can be proposed, at most, in a very restricted way, since in the list of about 1000 items which form the preserved thesaurus of Iberian,[2] only a few can be compared with Basque forms.[3] One case is the comparison, popular since W. von Humboldt, of Iberian town names formed with *Ili- Ild-* and Bsq. *iri, uri* 'town', but it can most probably be explained as a cultural diffusion.[4]

Without entering into a more precise definition of linguistic 'kinship', we acknowledge that languages can be related not only genealogically, but also by contact. We consider as related languages not only those descended from a common ancestor, but also languages which for historical reasons have had contact through migrations, conquest, proximity, commerce, etc. In both meanings of linguistic kinship, notwithstanding the Romance influence, Basque appears isolated.[5]

2. Thus Basque is a linguistic remnant of the remote pre-IE period, the only one left in western Europe. It would, then, seem possible that Basque is a vestige of an extended language, or family of languages, which has been confined to its present restricted area after having constituted something like *the* language of the West. This was the idea maintained by some scholars since, at least, the beginning of the eighteenth century, and which, from such Basque writers as Larramendi,[6] passed to W. von Humboldt.[7]

But probably the situation in western Europe was more complex. It is evident that the diffusion of languages and the establishment of monolingual areas are only possible under certain conditions. Political supremacy, cultural assimilation, very active channels of trade and military conquest are necessary conditions for the expansion of languages.

We do not believe that Basque is the residue of a language which was widespread. The study of place names seems to prove that the Basque language, or linguistic elements recognizable in Basque, are found in an

area extending along the Pyrenees, probably down to the coast of Catalonia,[8] and also west of Bilbao, through Cantabria and Asturias.[9]

Considering the situation in the West before the invasion of IE-speaking peoples, and considering also the fact that the regions in which Basque elements appear with more or less density never properly were cultural centers, we think that Basque represents the language of those regions alone, that is, in no way a wide-spread language. In any case, it is the only island emerging from the sunken world of pre-IE Europe.

3. To imagine the expansion of the IE-speaking peoples to the West, we have to start from the principle that the regions to the west of the Rhine were not originally populated by them. At the end of the Neolithic age (approximately before 2000 B.C.), an area bordered on the west by the middle Seine in France already contained tribes which can be considered with probability as the ancestors of IE speaking peoples. The British Isles, western Switzerland, the Ligurian and Tuscan regions of Italy, as well as Corsica, Sardinia and most of Sicily, remained untouched at this time by those cultures.[10]

If we reexamine again the western limits one millennium later of the archaeological cultures which we now know correspond to IE peoples, we see those peoples expanding around 1000 B.C. The Urnfields culture extends in a broad arrow covering the French Massif Central down to the lower Ebro in Spain, reaching to the Garonne valley and pointing to Spain through the passes of Navarre,[11] where the Basques lived. Apparently, by the same time, the same archaeological culture had developed in southern England.

We find that for approximately 1000 years the most active and expansive cultures of Central Europe had not conquered the West. Only with the movement of the Urnfields does a serious penetration begin to complete the Indo-Europeanization of the territories west of the Rhine, down to the mouths of the Tajo and Guadiana at the Southwest of the Hispanic peninsula, and north to the western shores of Ireland.

The importance and density of the pre-IE populations in the outermost western areas are proved by the fact that the IE features were deeply modified in the extant Celtic languages of Great Britain and Ireland,[12] and probably a similar process took place in northwestern Spain.[13] But the only language surviving from that linguistic substitution is Basque.

In one of his earlier books,[14] V. Gordon Childe pointed out the contrast not only with Central Europe and Scandinavia, but even with Great Britain, where, according to him, 'Aryans' appear: 'yet in France and the Iberian peninsula this [megalithic] civilisation shows not a trace of internal development, not a vestige of progress . . . Complete stagnation ruled in industry . . . This civilisation which stagnated on the Atlantic coasts for a thousand years or so, from the latter half of the IIIrd millennium B.C., was not European; Western civilisation was brought to the West by the Celts from Central Europe towards the end of the IInd millennium.'

The Indo-Europeans, attracted in the previous centuries by the riches

and splendor of the Near East, spread to the West only one millennium later. But against the prejudiced idea of Childe, we must emphasize that backwardness came to western Europe only after a period of creative activity and cultural irradiation. The 'soñoliento y decaído occidente'[15] in the face of the invaders of Hallstatt and La Tène, had been in former times a center of cultural innovations. Especially in the mining regions of Southern Spain the stimuli from the eastern Mediterranean world provoked an important development in metallurgy.[16] Without entering into the discussion of those very difficult chapters of European prehistory, we should at least examine the last important episode of a cultural diffusion originating in Spain and spreading throughout the West.

Archaeologists have pointed out the importance of the Beakers, the carriers of a type of ceramic (Bell Beaker, vaso campaniforme) which is found as far as northern Italy, western Yugoslavia, Bohemia and Moravia, Poland, the Rhine and Britain;[17] this ceramic has been discovered as far north as Scandinavia, but its origin is most probably found in the southern half of the Hispanic peninsula.[18] Of course the Beakers were imitated, and no doubt secondary centers developed, as seems certain in the regions of the lower Rhine and Great Britain.[19] Even a kind of 'reflux movement' extended IE influences up to the Atlantic shores.[20] It is important for us to point out that the Beakers were not actually a migrating people, but rather, it seems, prospectors for 'ores and precious substances'.[21] Even correcting the idea of a 'Beaker people', it remains certain that western elements, before the stagnation, were very important in culture and technology.[22] As F. C. Hibben sums up,[23] 'these Bell Beaker traders carrying with them their copper daggers and other bits of metal, are given credit for introducing the metal usage into many parts of Europe.'

4. Prehistorians seem to agree that following a period of activity originating in Spain, and extending to areas which were later conquered by IE-speaking peoples, after a long period of stabilization, there is again a movement in the opposite direction. As a survival of the prehistoric West, Basque would be expected to keep some tokens of those mutual influences. First we are going to consider some probable linguistic elements which appear both in Basque and in Indo-European; then, some mining terms, which are found in Basque and in IE languages, and which should prove the importance of the beaker bearers in diffusing cultural words.

C. C. Uhlenbeck[24] collected a series of words which show similarity in Basque and in Indo-European. In some cases the fact of an IE word being borrowed by Basque is quite possible: thus we would explain *(b)urki* 'birch' or possibly *(h)artz* 'bear'. The following could also be IE loans: *argi* 'light', like IE **arg-*, *(h)ar-* 'taken', like IE **gher-*, *arrano* 'eagle', like IE **er- or-*, *gar* 'flame', *gori* 'warm', like IE **g*^{w}*her-*, *tegi* 'place, house', as from IE **(s)teg-*, *il(l)e*, *ule* 'hair', like IE **wel-* 'wool'.

In other examples, the IE etymology being more obscure, it is possible that some words, found both in western IE dialects and in Basque, were borrowed from now extinct western languages; this would be the case with

andere 'miss, lady' as compared with Celtic forms, and other lexical items.[25] In other cases, since the IE parallels given by Uhlenbeck were too far away to the East (for instance Bsq. *gose* 'hunger' compared with Hitt. *kāsza*, Toch. *kaṣt* 'hunger'), the wisest course is to consider them only 'Anklänge' and not to use them for any conclusion.

A deeper insight into the relationship of IE to the neighboring languages, in order to broaden our understanding of the formation of Indo-European, could be reached by analyzing a suffix which appears both in IE and in Basque with the same values and entering a similar combination. This is the suffix *-ko* (and *-sko*)[26]. Giving briefly my results, *-ko-* is found throughout the IE area (Brugmann, *Grundr.* II 12.473ff., H. Hirt, *Indogerm. Gramm.* 3. 284). It could be analyzed as derived from the 'Erweiterung' (Specht) or 'élargissement' (Benveniste) *-k-*, which took a vowel.

-ko- appears[27] both in primary formations of the type Gk. *thḗkē* 'chest', Skt. *dāhka-* 'deposit', and in secondary ones: OHG *hus-in-ga* 'penates'. As it was usually added to vowels, the types *-ako-*, *-eko-*, etc. developed, *-ĭko-* becoming especially frequent in the type Gk. *hippikós* and OHG *wintarig*.

In Basque, *-ko* forms one of the genitives: *etxeko teilatua* 'the house's roof', *atzoko euriak* 'yesterday's rainfalls', but it also forms derived nouns as in: *garriko*, *gerriko* 'girdle', from *gerri* 'waist', *soineko* 'dress', from *soin* 'body'.

In a parallel way, *-ko* forms diminutives in Indo-European as in Basque: compare Skr. *maryaká-* 'little man', *áśvaka-* 'little horse', *yuvaśá-* or *yuvaká-* 'young man', OIr. *óac*, Goth. *juggs* 'young man', with Bsq. *idiko* 'young bull', from *idi* 'ox', *mandako*, *mandoko* 'little mule', from *mando*.

The parallel becomes much more striking when we discover in IE the conglutinate *-sko-*: OHG *rasc* 'quick', AS *horsc* 'clever', and especially adjectives like Goth. *barnisks* 'childish', OSl. *nebesiskŭ* 'heavenly', just as in Basque *urrezko* 'golden', *pozezko* 'contented'. But in Basque the suffix could be analyzed as instrumental suffix *-z* (so *urrez* means 'with gold') plus derivative *-ko*[28].

The suffix *-ko-* in IE is very much alive, as is shown in the extensive area in which it is found. Its importance in Basque would prove that it is primitive in the language. Are we allowed to maintain that it is a western element? We could answer that the suffix *-k(o)-* was found in the old world in a way which has been faithfully continued both in Basque and in IE languages (but, apparently, not in Hittite).

Thus we see that Indo-European could be formed with elements of different origins. Several related dialects[29] which expand through commerce, military conquest, or cultural attraction, are imposed on linguistically alien populations and absorb elements of various origins. The fact that the suffix *-ko-* is extended to the whole range of IE languages, except Hittite, would favor the idea of common Indo-European having existed, whereas its presence in at least[30] one non-IE language as Basque, would supply an argument to those scholars who imagine the formation of Indo-European rather as a crystallization by mixture of different elements.[31] What is important is to

realize that *-ko-* belonged to the IE common stock when the IE migrations started toward India and toward Greece in the third millennium.[32]

Furthermore, the traditional idea of a splitting of Indo-European[33] with no consideration given to time must change as we view the process from the West. In contrast with the formation of separate branches of Indo-Europeans who migrate (Greeks, Indo-Iranians), the peoples of the western area seem to remain undivided. This is still the time of Krahe's 'Old-European', a stage in which Latin and Celtic, Italic and Venetic, Germanic and Illyrian, languages which will emerge into the historical light in the following millennium, are scarcely differentiated and still lack their later characteristics.

5. Let us now study the names of two metals which are typically cultural words and which keep the traces of those early metallic diffusions from the Hispanic peninsula into Central Europe and the Mediterranean shores. In the case of silver we could even discover the primary oriental origins of the word, its traces in Basque and perhaps in Iberian, and finally its survival in the north of the Indo-European territory.

That silver was abundant in ancient Spain is known by the classical tradition.[34] The greatest native silver bed in the West was at Almizaraque (in the Vera district of Almería), but according to M. Gómez-Moreno,[35] it was among the deposits of carbonates mixed with silver and copper at neighboring Sierra Almagrera that the exploitation of silver first began in the ancient world. Silver was first brought to the eastern Mediterranean by the Phoenicians.[36]

In most of the IE languages[37] silver received its name from its brilliancy, **arg-* meaning first 'white', as in Skt. *rajatá-* 'whitish', *árjuna-* 'white', Gk. *argḗs* 'bright', Hitt. *harkiš* 'white'. Most of the languages in a range from Tocharian and Avestan to Irish formed a derivative in *-nt-* to indicate the precious metal: Av. *ərəzata-*, Toch. A *ārkyant*, OP *ardata-*, Arm. *arcatʻ*, Lat. *argentum*, O. *aragetud*, OIr. *argat*, W. *arian(t)*. Greek has a special derivation, *árguros*, and Mess. *argorian* must probably[38] be connected with the Greek form. To the East, in addition to the cited form, Tocharian A shows the alien *nkiñc*, *ñikañce*.[39]

All the Germanic and Balto-Slavic languages have a special, evidently non-IE word: Goth. *silubr*, AS *siolfor*, *seolfor*, OHG *sil(a)bar*, ON *silfr*, Lith. *sidābras*, OPr. (acc.) *sirablan*, OSl. *s(ĭ)rebro*. According to O. Schrader,[40] the Germanic form has passed to Lapp. *silbba*, but he misinterprets J. Grimm when saying that he derives Bsq. *cidarra* from Visigothic.[41] An Alpine place name *Silvretta* has been pointed out by Hubert, Feist, Schrader-Nehring.[42]

The Bsq. forms *zil(h)ar*, *zillar*, *zidar*, *zirar*[43] are really close to those found in northern IE, and they explain in the most clear way how a line of diffusion started from Spain during the early Bronze Age and reached the regions of central Europe where IE languages were spoken.[44] Unfortunately we are not certain that we know the Iberian word for silver, but

ṣalir could be a good confirmation for Bsq. *zil(h)ar*, being well rooted in the peninsula.[45]

The comparison of Bsq. *zil(h)ar* with Assyrian *ṣarpu*, which H. Hommel established,[46] must be examined in connection with other Semito-Hamitic and African forms: Arabic *ṣarīf*, Berber *azref* (Sous), *azerfa* (Mzab), Hausa *azurfa*.[47] Thus the word has been attested in the East, when the archaeologists derive the early knowledge of minerals in Iberia. Perhaps the ingenious discovery of H. Brunnhofer[48] of the passage of Panyasis in Steph. Byz. s. u. *Tremílē*: *Síbrōi* (*Simbrōi* one ms.) *ep' arguréōi potamôi* might be a proof of the presence of this word in Asia Minor as well.

6. Let us now examine the IE words for lead. First the Lat. *plumbum*, with Gk. *mólibos*, *mólubdos* (Epid. *bolimos*, Att. *bólubdos*, Rhod. **bolibos*).[49] Then we have the northwestern form OIr. *luaide*, AS *lēad*, MHG *lōt*.[50] A third form is shown in the Germanic territory: OHG *blīo*, ON *blý*. Each of the other words has an area of its own: Lith. *švinas* (which appears in Slavic as well), OSl. *olovo*, Skr. *sīsa-*, Av. *srva-*.[51]

The etymological dictionaries put together *plumbum* and *mólubdos*, and all agree on a non-IE source: Ernout-Meillet[52] speaks of a 'Mediterranean language' and they add: 'ibère? le plomb venait d'Espagne.' Walde-Hofmann[53] considers the word a borrowing 'aus einer nicht näher bestimmbaren mittelmeerländiche-iberischen Quelle . . . , aus der auch georg. *brpeni*, *prpeni* "Blei, Zinn"[54] und bask. *berun* "Blei" stammen.' H. Frisk[55] coincides in emphasizing the importance of lead in the West, especially in Spain, and discusses the possibility of connecting the various Greek forms with Bsq. *berun*.

There are attempts to explain *berun* from purely Basque roots (for instance from the adjective *bera* 'soft', but the ending then remains unexplained). But the connection with other languages seems more probable, since metal names are typically cultural words. Not to be forgotten are some non-Arabic Berber forms: *buldun* (Ṣalaḥ), *aldun* (Kabilya) 'lead', and *aldun* (Sous) 'tin'.[56]

G. Bähr[57] reconstructed a form **bolum*, which could explain Bsq. *berun* in its connection with *plumbum*, *mólubdos*. A form **belum* is also legitimate,[58] and as Basque voices the initial stops and makes anaptyxis, **plum* would be at the basis of *plumbum*, and possibly of the Celtic and Germanic forms, if by supposing the loss of initial *p* in Celtic, **loudho-* be considered from the same origin.[59]

The sound ^{m}b which A. Martinet[60] has supposed for Basque, and which could exist in Iberian,[61] would better explain the changing initials of Gk. *mólubdos*, *bolimos*.[62]

Leaving aside the other Germanic type OHG *blīo*, which is a color noun in connection with *blue*,[63] the other Celtic-Germanic form could be reduced, as O. Schrader indicated,[64] to the Latin word: *plumbum* could, according to him, be analyzed as from **plon-dho-*, and to it could be equated a Celtic root **loudho-* which was still seen in Walde-Pokorny 2.442, but now has disappeared from Pokorny's *IEW*.[65]

Two other proposed etymologies for the Greek forms for lead, by V. Pisani,[66] and by A. van Windekens,[67] do not seem very tempting.

Lead, like silver also appears late in central and northern Europe.[68] It was found especially in the West: Spain[69] and Great Britain.[70]

If the word *silver* well represents the activities of the Beaker prospectors in the directions of central and northern Europe, lead proves also those activities in the direction of the East. We do not need to suppose that they personally carried the name as far as Greece; the western name in this case travelled together with the product.

Notes

[1] A. Tovar (with K. Bouda, R. Lafon, L. Michelena, W. Vycichl and M. Swadesh), El método léxico-estadístico y su aplicación a las relaciones del vascuence, *Bol. de la R. Soc. Vasc.* 17.249–81. Of course, I consider untenable the first expectations of glottochronology and its translation in terms of time. But still the fact remains that a list of well-selected words reflects in a countable manner the relationship of languages. Provided that the similarities are not very few (because in that case they could be pure chance), they express, even in a crude manner, a relationship between languages. That is my answer to the question posed by Prof. H. Vogt, *Norsk Tidsskrift for Sprogvid.* 20.28 ff.

[2] See my Léxico de las inscripciones ibéricas, *Estudios dedicados a Menéndez Pidal* 2.273–323. To these the new materials from M. Gómez-Moreno, *La escritura bástulo-turdetana (primitiva hispánica)* (Madrid, 1962) must be added.

[3] A. Tovar, *Archivum* 4.220–31 (Oviedo, 1954), reprinted in the author's book *El euskera y sus parientes* (Madrid, 1959). See also L. Michelena, *Bol. de la R. Soc. Vasc.* 7.495–503, *Archivum* 8.33–47 (Oviedo, 1958), with new discussion of the problem.

[4] O. Menghin, *Runa* 1.160 (Buenos Aires, 1948). On the Iberian names with *Il-* cf. U. Schmoll, *Die Sprache* 6.46–55.

[5] P. Bosch-Gimpera, *El problema indoeuropeo* (México, 1960) 119 expresses the common opinion of archaeologists according to whom the Basques descend from pastoral neolithic Pyrenaean culture, covering with the Pyrenees the Cevennes and the French Maritime Alps. That Basque could be brought by later migrations seems reasonably excluded; cf. Bosch-Gimpera, op. cit. 160n316. For a more recent examination of the question, see Problemas de la prehistoria y de la etnología vascas, *IV Symposium de Prehistoria Peninsular* (Pamplona 1966).

[6] *De la antigüedad y universalidad del Bascuenze en España* (Salamanca, 1728), and again in the introduction to his *Diccionario Trilingüe* (San Sebastián, 1745).

[7] *Prüfung der Untersuchungen über die Urbewohner Hispaniens vermittelst der Vaskischen Sprache* (Berlin, 1821). A new Spanish translation under the title *Primitivos pobladores de España y la lengua vasca* (Madrid, 1959) has been done by F. Echebarria.

[8] J. Corominas, Toponímia d' Andorra, *Recueil Brunel* 1.288–310 (Paris, 1955), Sobre els elements pre-romans del domini català, *Act. y Mem. VII Congr. Intern. Ling. Románica* 2. 401–17 (Barcelona, 1955).

[9] A. Tovar, *Cantabria prerromana* (Madrid, 1955) 11ff.

[10] Bosch-Gimpera, *El problema indoeuropeo* Map III, and G. Devoto, *Origini indeuropee* (Florence, 1962) map on p. 80. It is very interesting to compare the map of the megalithic culture in V. G. Childe, *Prehistoric Migrations in Europe* (Oslo, 1950) 111, or that of Stuart Piggott, *Ancient Europe from the beginnings of Agriculture to Classical Antiquity* (Chicago, 1965) 61.

[11] I use the maps of V. G. Childe, *Prehist. Migr.* 142, IX of Bosch-Gimpera, *op. cit.*, Devoto 154, Piggott, *Ancient Europe* 146.

[12] The most complete collections of possible relations are found in the series of papers by J. Pokorny, Das nichtindogermanische Substrat im Irischen, *ZCPh* 16.95–

144, 231–66, 363–74, 17.373–88, 18.233–48; see also by the same author Zum nichtindogermanischen Substrat im Inselkeltischen, *Die Sprache* 1.235–45, The Pre-Celtic Inhabitants of Ireland, *Celtica* 5.229–40. H. Wagner, in his monograph *Das Verbum in den Sprachen der britischen Inseln* (Tübingen, 1959), enlarges the field of these comparisons.

[13] A. Tovar, *Journal of Celtic Studies* 1.19f. (Philadelphia, 1949) or *Estudios sobre las primitivas lenguas hispánicas* (Buenos Aires, 1949) 205f.

[14] *The Aryans, A Study of Indo-European Origins* (New York, 1926) 210f.

[15] Pia Laviosa Zambotti, *España e Italia antes de los romanos* (Madrid, 1955) 280. P. Bosch-Gimpera in his paper Culture megalítica portuguesa y culturas españolas, *Revista de Guimarães* 76 (1966), offprint of 60 pages, has revised this period of 'probably three millennia' which extends from about 4500 B.C. through the ages which he calls Caves culture, initial aeneolithic and Beaker culture. The most brilliant period of the Poruguese megaliths and the Spanish Beakers is, according to him, the third millennium down to the early centuries of the second, when the Bronze age began.

[16] This opinion has been common for about forty years, when the archaeological type was studied by P. Bosch-Gimpera and A. Castillo. Now doubts are sometimes expressed about its origin; so H. Hencken (of whose paper Indo-European Languages and Archaeology, *American Anthropologist* Memoir No. 84, 1955, we make large use) says (p. 10): 'allegedly but by no means certainly from Spain.' But the origin and diffusion of the ceramic type from the Hispanic peninsula remain certain, even with the recognition of important secondary local centers: Bosch-Gimpera, *El probl. indoeur.* 132ff. I copy from the recent book *Prehistoric Societies*, by G. Clark and S. Piggott (in the series *The History of Human Society*, ed. by J. H. Plumb, New York, 1965) 291: 'The Iberian origin of this remarkable *diaspora* seems inevitable.' But the hypothesis of a brachycephalic race as characteristic of a Beaker people vanishes; see M. A. Smith, Iberian beakers, *Proc. Prehistoric Society*, n. s. 19. 95–107 (Cambridge, England, 1953).

[17] S. Piggott, *Ancient Europe* 101.

[18] G. Clark and S. Piggott, *Prehist. Soc.* 290 point out the importance of oriental 'colonies', which developed metallurgy in Spain and Portugal; cf. also Piggott, *Ancient Europe* 76. This can be extremely significant for our argument.

[19] H. Hencken, *op. cit.* 10f., S. Piggott, *Ancient Europe* 100–02.

[20] 'In central and west-central Europe the makers of Bell Beakers found themselves in a region in which long-standing traditions of Danubian and allied ancestry were now mixed with, and in some areas dominated by, the steppe-derived cultures archaeologically denoted by vessels of Corded Ware, stone battle-axes and such other features as individual burial under a tumulus (Clark and Piggot, *Prehist. Soc.* 293). As in contact with those central European cultures, Indo-European influences touched the Bell Beaker bearers, 'the reflux movement may well have also spread IE dialects into western Europe. . . The distribution of river names belonging to the non-Celtic but IE substrate language . . . would be best explained by referring them to the folk movements involved in the Bell Beakers reflux' (Clark and Piggott 294, cf. Piggott, *Ancient Europe* 102).

[21] Childe, *Aryans* 99. Cf. Piggott, *Ancient Europe* 100, 118.

[22] M. Gómez-Moreno, *Misceláneas* (Madrid, 1949) 71f.

[23] *Prehistoric Man in Europe* (Norman, Oklahoma, 1958) 187. The argument finds support in the pages of P. Kretschmer, *Glotta* 32.13ff. where he traces the diffusion of the Beakers following the terms Norax (the Iberian colonizer of Sardinia), *nuraghe*, the Austrian *Noreia*, and the Germanic words for ore: OHG *aruz(zi)*, *erizzi* 'Erz', which would be forms with 'Iberian' prothesis corresponding to Skt. *lohá-* 'reddish metal, copper, iron', Lat. *raudus* 'ingot', Sl. *ruda* 'ore', OIc. *rauðr* 'rotes Eisenerz', etc. (Pokorny, *IEW* 873). These words still permit us to trace a way going from Spain through Sardinia (a tradition, as it seems, still reflected in Pausanias and Solinus) up to central Europe. Passing through Sardinia, 'die hispanische Siedler dürften auch die Kupfergewinnung in Noricum gelehrt und eingerichtet haben', as Kretschmer (16) says.

[24] *Anthropos* 35/36.202–07.

[25] A. Tovar, *Bol. de la R. Soc. Vasc.* 1.31–39 (reprinted in *Estudios*). See also N. M. Homer, *ibid.* 6.399–414.

[26] A. Tovar, *Archivio glottol. italiano* 39.56–64, partly reprinted in *El euskera y sus parientes*.

[27] It must be noticed that *-ko-* did not reach Hittite, although it is largely represented in Sanskrit. This would be a proof of its western origin and for the old age of the separation of the Hittite branch.

[28] H. Schuchardt, *Die iberische Deklination* (Academy of Vienna, 1907) 66ff. and V. Bertoldi, *Studi Etruschi* 7.284 already compared to the Basque *-z-ko* the 'Ligurian' suffix *-asco*.

[29] I try to be careful in formulating this statement, to leave open the possibility of 'common IE' never having existed except as a necessary scientific abstraction: see references to this question in A. Scherer, *Kratylos* 1.3f.

[30] Other possible, but doubtful, comparisons in *Arch. glott. ital.* 39.62f. (or *El euskera y sus parientes* 73f.).

[31] This idea seems very tempting, as it was formulated first by C. C. Uhlenbeck, *Oer-indogermaansch en Oer-indogermanen, Mededel. d. kon. Akad. v. Wetensch. Afd. Letterk., Deel* 77, *Ser.* A (Amsterdam, 1935), and again, in a shorter redaction, in *Amer. Anthropologist* 39.385–93; N. S. Trubetzkoy in his Gedanken über das Indogermanenproblem, *Acta linguistica* 1.81–89, comes close to this idea of IE being originated from at least two different layers of languages: Uralic on one side (the 'analogic' component A of Uhlenbeck) and Caucasian on the other (the 'anomalistic' B of that scholar). Why not discover in Basque the traces of the Western languages, or a 'third component'? I would not dare, even after studying the deep and exciting considerations of H. Wagner (see n. 12), to characterize this 'third element'. Let us now point out that it penetrated as far as Sanskrit and contributed to the parent language still in the third millenium.

[32] The linguists of some years ago now seem timid concerning the antiquity given for IE invasions. Not only does Greek in the Mycenaean tablets compete with the oldest records in Hittite and with the first testimonies of the Aryans on the borders of Hittites and Assyrians, but archaeologists are beginning to discover the invasion of the Greeks in layers belonging to the Early Helladic, in the last centuries of the third millenium, according to the interpretation my colleague W. P. Donovan gave in a public lecture (Feb. 1966) of the results of excavations at Lerna, as published by Prof. J. L. Caskey, *Hesperia* 29.285–303.

[33] As it appears, for instance, in the classical exposition by A. Meillet. I would like to quote among the numerous papers by H. Krahe on the subject of 'Alteuropäisch' one of his last publications, in which he explains how he passed from the idea of a particular 'pre-Celtic' language to that of nondifferentiated Western IE: 'Vom Illyrischen zum Alteuropäischen', *IF* 69.201–12.

[34] O. Schrader, *Sprachvergleichung und Urgeschichte* 2.85 (Jena, 1906). From the ancient testimonies on Spanish silver I should choose, after Blümner, *RE* 2A.18, Pseudo-Aristotle, *De mir. ausc.* 135, 844[a]17 (the Phoenicians coming back from Tartessos were not able to carry all the silver they had won, and so they were obliged to make of silver everything used at the ship, even the anchors), Diodorus 3.36.1 (the Iberians, according to a report, learned from the Phoenicians the properties of silver, and so developed mining), Strabo 3.2.14 p. 151 (giving as proof of the riches of the country that when the Carthaginians first entered Turdetania with [Hamilcar] Barca, they used silver mangers and large silver jars for storage; Strabo thereafter refers to the famous king *Arganthonius*, the Silver-man).

[35] *Miscel.* 71. See the results of a research on the place by P. Bosch-Gimpera and F. de Luxán, Explotación de yacimientos argentíferos en el eneolítico de Almizaraque, *Investigación y progreso* 9 (1936) 112ff. R. J. Forbes *Studies in Ancient Technology* 8 (Leiden 1964) 203 says explicitly: 'Deposits of these [silver] ores are rare; apart from rich deposits of this type in the New World, they are found in the deposits of lead ores in Spain and in parts of Cornwall and Hungary, but they are of no impor-

tance in the Near East.' See the same writer, p. 206 and 226, for the silver and lead mines on ancient Spain, with some skeptical views about Tarshish-Tartessos p. 211.

[36] Blümner, *RE* 3A.14, Schrader-Nehring, *Reallex. der idg. Altertumskunde* 2.393, Gómez-Moreno, *Miscel.* 71, 145. But Childe, *Prehistoric Migrations* 71, 199, finds silver in the Cycladic culture and in Crete about 2200 B.C. J. Graham D. Clark in his *Prehistoric Europe, The Economic Basis* (London, 1952) 196 says: 'Both lead and the silver with which it is commonly associated in nature, were used at a comparative early date in the eastern Mediterranean, though the lead used at Mycenae may well have come from Anatolia. In Iberia lead appeared only in small quantities at El Argar and El Oficio. . . In temperate Europe the use of lead appears to have been an innovation of the Late Bronze Age.' But K. Branigan, *AJA* 72.221ff. has set the silver and lead in prepalatial Crete on the background of the Aegean archaeology and he emphasizes (p. 224) that 'the place which silver and lead working occupied in Minoan metallurgy was small compared with that of both copper and gold working.' The metallurgists of the Cyclades 'obtained silver from the islands' (p. 226), but he acknowledges (p. 227) 'the possibility that from EM III onward the Minoans obtained some of their metal from Italy, Sardinia or Spain.'

[37] Schrader-Nehring 2.393f., O. Schrader-H. Krahe, *Die Indogermanen* (Leipzig, 1935) 40. For the linguistic forms see J. Pokorny, *IEW* 64 (or Walde-Pokorny 1.82), C. D. Buck, *Dict. of Selected Synonyms* §9.65, G. Devoto, *Origini, Tavole*, no. 603, who notes (p. 240) that *aes* and *argentum* are 'forme popolari, estranee alla tradizione della classe dominante, note ma non-accolte nei riti.'

[38] W. Porzig, *Die Gliederung des idg. Sprachgebietes* (Heidelberg, 1954) 143.

[39] Ibid. 212. The Hesychian gloss *skárkē: Thrakistì argúria* (4.40 M. Schmidt) remains mysterious. Let us point out *The Oxford Dict. of English Etymology* (Oxford 1966) by C. T. Onions *et al.* says that the words for *silver* are 'perh. all ult. of Oriental origin.'

[40] *Sprachvergleichung und Urgeschichte* 2.53.

[41] Schrader seems to rely here on J. Grimm, but I have checked the text he quotes (*Gesch. der deutschen Sprache*, Leipzig 1853, 8) and there he simply asks: 'auffallender ist die verwandschaft zwischen *silabar* und bask. *cilarra*. . . . Mag man noch am westgoth. einflusz glauben?' In Grimm's *Deutsches Wörterbuch* X 1 (Leipzig, 1905) 974f. the Basque form *zillar* appears among the non-IE names of silver. The connection of Basque and Germanic remains, but rather in the opposite way. The similarity is pointed out but with no explanations by W. J. van Eys in his *Dictionnaire basque-français* (Paris, 1873) s. u., M. de Unamuno, *ZRPh* 17.143, J. Caro Baroja, *Los vascos* (San Sebastián, 1949) 255, J. Whatmough, *Harvard Studies in Class. Philol.* 60.175f. This author says that the word's source 'may have been Alpine or "Mediterranean" ' —but he adds resolutely 'not, I think, Iberian.'

[42] References in J. Whatmough, *Dialects of Ancient Gaul* (Microfilm, Ann Arbor 1948–51) 1197.

[43] H. Gavel, *Rev. Intern. de Est. Vascos* 12.244f. (who very cautiously says that 'le mot paraît être apparenté au type germanique'), L. Michelena, *Fonética histórica del vasco* (San Sebastián, 1961) 315.

[44] See the map 2 in Pia Laviosa Zambotti's *España e Italia*, facing p. 140.

[45] M. Gómez-Moreno, *Miscelánea*s 278 pointed out the form *ṣalir*; cf. A. Tovar, *Léxico de las inscripciones ibéricas* 317.

[46] *Archiv für Anthropologie* 15 Suppl. 164f. The usual term for silver in Assyr. is *kaspu*, see F. Delitzsch, *Assyr. Handwörterbuch* (Leipzig, 1896) 345a; for *ṣarpu* see 574b. Also in Arabic the most common word for silver is not *ṣarīf*, but *feḍḥḍha*. The arguments in favor of Hommel's comparison are found in F. Kluge, *Etym. Wb.* der *deutschen Sprache*[19] 709: 'Die unregelmässigen Lautentsprechungen zeigen, dass es sich um kein bodenständiges Erbwort handelt, sondern um ein nichtidg. Wanderwort, das Schmiede etwa aus Vorderasien (assyr. *ṣarpu*) gebracht haben mögen.'

[47] D. Wölfel, *Eurafrikanische Wortschichten als Kulturschichten* (Salamanca, 1955) 129. These comparisons are already found in A. Trombetti, *Origini della lingua*

basca (Bologna, 1925) 148, who emphasizes that 'il tedesco *Silber* deriva dall'iberico, non viceversa.' The same idea is already presented by H. Schuchardt, *Rev. Intern. de Est. Vascos* 7.304, against the word having being carried into Spain and Africa by Visigoths and Vandals. One aspect of Trombetti's explanation, which I cannot judge (the etymology of the Semitic form being from Akkadian *ṣararu* 'illuminate', Arabic *saru-ha* 'it was sheer, clear'), would support an original name in the Semitic civilized areas of the Near East. Other ancient names for silver can be found in the interesting chapter on this metal and lead by R. J. Forbes, *Ancient Technology* 8.200f.

[48] *Urgeschichte der Arier in Vorder- und Centralasien* (Leipzig, 1879) 2.7ff. On this same relationship, F. Ribezzo, *Arch. glott. ital.* 35.53, and especially in his paper Il nome lidio-ligure dell'argento, *Archiv Orientální* XVIII 4.243–247. He quotes K. Oštir's monograph *Drei vorslawisch-etruskische Vogelnamen* (Lubljana 1930), in which a proto-Basque form *zilphar is postulated. V. Hehn's explanation (*Kulturpflanzen und Hausthier*, Berlin 1911, 575), supposing on *Alybe*, The Homeric 'native place of silver' (B 857), a **Salybe* as an origin for the word *silver* is so gratuitous as to put the form in connection with the *Chalybes*. G. Ipsen, *Festschrift Streitberg* (Heidelberg 1924) 229f. pronounced himself for a derivation of *silver* from Lydian or some other Anatolian language.

[49] Concerning a Mycenaean *moriwodo* which Frisk mentions with a question mark (*KN Og*, 1527.1, 2, 3) there is no evidence to deny that it is the oldest record of *mólubdos*, but the testimony remains isolated and in a fragmentary tablet.

[50] Buck, *A Dict. of Selected Synon.* § 9.68. H. Krahe, *Sprache und Vorzeit* (Heidelberg 1954) 122, 140 points out that this lexical Celto-Germanic community for the lead name is also repeated for the iron.

[51] Buck, *loc. cit.*

[52] *Dict. étym. de la langue latine*[4] 516. Compare V. Bertoldi, *Arch. glott. ital.* 31.95, who speaks of Iberia as 'terra della più antica espansione del piombo . . . da occidente verso oriente.' The same scholar, *Nueva Rev. de Filol. Hispánica* 1.144ff. put the word *plumbum* in the same context as *galena* (read in lead ingots from Cartagena, *CIL* 2,284) and *minium* (cf. A. Tovar, *Beiträge zur Indogermanistik und Keltology* J. Pokorny zum 80. Geburtstag gewidmet, Innsbruck, 1967, 107ff.).

[53] *Lat. etym. Wb.* 2.326.

[54] The Georgian form seems really very different; so far as we know, nobody among the specialists, neither Trombetti, nor Bouda, nor Lafon, has compared this word with the Basque word.

[55] *Griech. etymol. Wb.* 2.251f., cf. Boisacq[4] 644.

[56] D. Wölfel, *op. cit.* 130.

[57] *Rev. Intern. de Est. Vascos* 24.26.

[58] See V. Bertoldi, *Arch. glott. ital.* 31.96. For the regular passage of intervocalic *l* to *r* in Basque, Michelena, *Fonét.* 311ff. He supposes that Biscain *beraun* would be the primitive form, against the reduction *berun* (p. 97), but we cannot see the basis of his argument. If we compare *garau* < Lat. *granum*, *beraun* would suppose **beranu(n)* (?).

[59] Such is the hypothesis made by Schrader, *Sprachvergl. u. Urgesch.* 2.95, who derives OIr. *luaide* from **plundho-*, and supposes that the Germanic type *lead* comes from the Celtic. Walde-Pokorny 2.442 asked if the form *loudho-* (if not from **pleu-plou-*, see also Kluge, *Etym. Wb. der deutschen Spr.*[19] 447) could be related with OCS *olovo*, but Pokorny avoids in his *IEW* these difficult questions.

[60] *Économie des changements phonétiques* (Berne, 1955) 387f., see also L. Michelena, *Homenaje a Martinet* 1.153f., *Fonética hist.* 270f.

[61] A. Tovar, *Homenaje a Martinet* 3.175.

[62] Characteristic of the Basque and Latin forms appears to be the presence of a nasal which is lacking in the Hebr. *oferet*, Sum. (doubtful) *abar*, (add Arabic *abār*), which according to Schrader-Nehring 1.151 'klingen merkwürdig an das bask. *berun*'.

[63] So, with some doubt, Walde-Pokorny 2.210, Pokorny 155.

[64] *Sprachvergl. u. Urgesch.* 2.95.

[65] To resolve the problem of the nasal, Schrader invokes Germanic cases of its presence or absence, like Goth. *dumbs/daubs* and other similar radicals in Noreen, *Abriss der urgerm. Lautlehre* (Strassburg 1894) 210f.

[66] *Rev. des études anciennes* 37.154, in connection with Gr. *molúnō*, *mélas*, with *b* from Celtic origin. Similar derivation from *mélas* is supposed by V. Georgiev; see for this and other etymologies D. A. Hester, *Lingua* 13.360.

[67] *Le pélasgique* (Louvain, 1952) 122f., thinking of the color words from **bhel-*.

[68] Schrader-Nehring 1.149f.

[69] Cf. Pliny 4.112, 34.138, particularly Baetica, Pliny 34.164f.; on the importance of the lead mines at Castulo, Strabo 3.2.10 p. 148.

[70] Blümner, *RE* 3.561. On the distribution of lead in the old West, see the indications of Clark transcribed above, n. 36. Unfortunately I could not see the monograph of K. Oštir, *Vorindogermanische Metallnamen in Alteuropa*, which appeared in volume 4 of *Arhiv za arbanasku starinu* (about 1926).

The Origins of Settled Farming in Temperate Europe

Bernard Wailes

I. Introduction

It is my intention to summarize the evidence regarding settled agriculture, as opposed to transitory (or shifting) agriculture, in Temperate Europe prior to the Iron Age. This whole problem has received relatively little attention in the archaeological literature, but seems potentially of considerable importance since (a) the change from transitory to settled agriculture might be associated with changes in sociopolitical life, (b) settled agriculture could carry a terminology that might be reflected linguistically, and (c) economic evidence will improve our understanding of the cultures involved. With experience in archaeology, and not in linguistics, I shall concern myself with the archaeological evidence.

Fortunately, some evidence for prehistoric agricultural activities can be recognized both archaeologically and palynologically. Soil studies may play their part also, although little comprehensive work of this nature has yet been carried out. As a starting point, Clark (1952:Ch. IV) provides a convenient summary up to 1952. Over much of Temperate Europe the earliest recognized Neolithic is associated with shifting 'slash-and-burn' agriculture, recognizable in pollen sequences by sudden, though temporary, 'clearance horizons' in which tree pollen is drastically reduced (especially the slow-regenerating 'Mixed Oak Forest' components) while herb and grass pollen rises correspondingly. This is normally followed by forest regeneration, demonstrating the abandonment of agriculture in the locality, except on some areas of poor soil, where it is suspected that natural regeneration of forest cover might have been impossible, or nearly so.[1] By the Late Neolithic, however, various lines of evidence show that clearance in some areas at least was permanent, suggesting the introduction of more settled agriculture or, alternatively, a shift in emphasis toward stock-breeding, that would equally result in a prevention of forest regeneration.

Since 1952 a considerable body of evidence has accumulated to throw more light upon this question. Stock-breeding, by its nature, is difficult to assess, particularly quantitatively in relation to the total rural economy. But agriculture leaves more permanent traces for recovery under favorable conditions, and also can show more clearly the transitory or settled nature of the economy: it is for these reasons that I discuss it. I also include a summary of direct evidence for ploughing prior to the Iron Age, since it is

likely that ploughing and settled agriculture are intrinsically related in Temperate Europe (although the former does not automatically imply the latter), and there is no satisfactory evidence to associate the use of the plough with 'slash-and-burn' shifting cultivation.

The following sections consider briefly the several types of evidence, and review the dating. Details of sites referred to are given in Appendices 1–4. A chronological table (Fig. 1) and a distribution map (Fig. 2) are included. Appendix 5 summarizes the redating of the 'Deverel-Rimbury' complex, formerly considered to be Late Bronze Age: this question is an important one for the dating of much of the British evidence cited. Radiocarbon dates are quoted as calculated on the 5568 ± 30 year half-life, with the recalculation according to the 5730 ± 40 year half-life added in brackets.[2]

II. Ploughs

Nothing of importance to our purpose can be added to Clark (Ch. IV), although Müller-Wille (1965) provides a more up-to-date source now. All ploughing in Temperate Europe earlier than the Iron Age appears to have been done with an ard (scratch plough), as opposed to the 'heavy' or 'true' plough (i.e. with a moldboard). Several pre-Iron Age ards, and representations of ards, are known; none can be assigned to a period earlier than the Late Bronze Age.[3]

III. Ploughing

Appendix 1 lists 33 sites at which ard furrows have been recognized. Normally furrows are recognized where the share penetrates through rather thin topsoil into a subsoil of a contrasting color. In such cases careful excavation may reveal the bottoms of the furrows showing, in plan, as lines of contrasting color against the subsoil (usually darker topsoil, lighter subsoil). At all but three (Nos. 29, 31, 33) of the 33 sites listed the furrows were discovered underneath barrows. Patzold (1960) maintains that this is the result of a ritual practice of ploughing the site upon which a burial mound was to be built, as an initial stage of the burial ritual. This might possibly be so in some cases, but it seems equally plausible to explain most examples as accidental preservation of an old plough soil from later cultivation and erosion that would probably, in most cases, completely destroy prehistoric furrows. In any case, the use of a plough for ritual purposes would strongly suggest a knowledge and practice of plough agriculture on the part of the community responsible. Many of the discovered instances are rather fragmentary, but most appear to represent 'cross-ploughing' (see Clark 1952:99,103–5; Megaw et al. 1961:206–7). Only one example (No. 8) seems to show 'one-way' ploughing fairly clearly, and this is a small exposure, ca. 10 meters long, with furrows spaced at curiously wide intervals. This evidence for the prevalence of 'cross-ploughing' is completely in keeping with the evidence for the use of the ard (see Section II). There is some evidence for 'tilting' the ard (Megaw et al. 1961:207, 212, and refs.), which will 'throw' some sort of a ridge (Payne 1947:90): it should not be thought

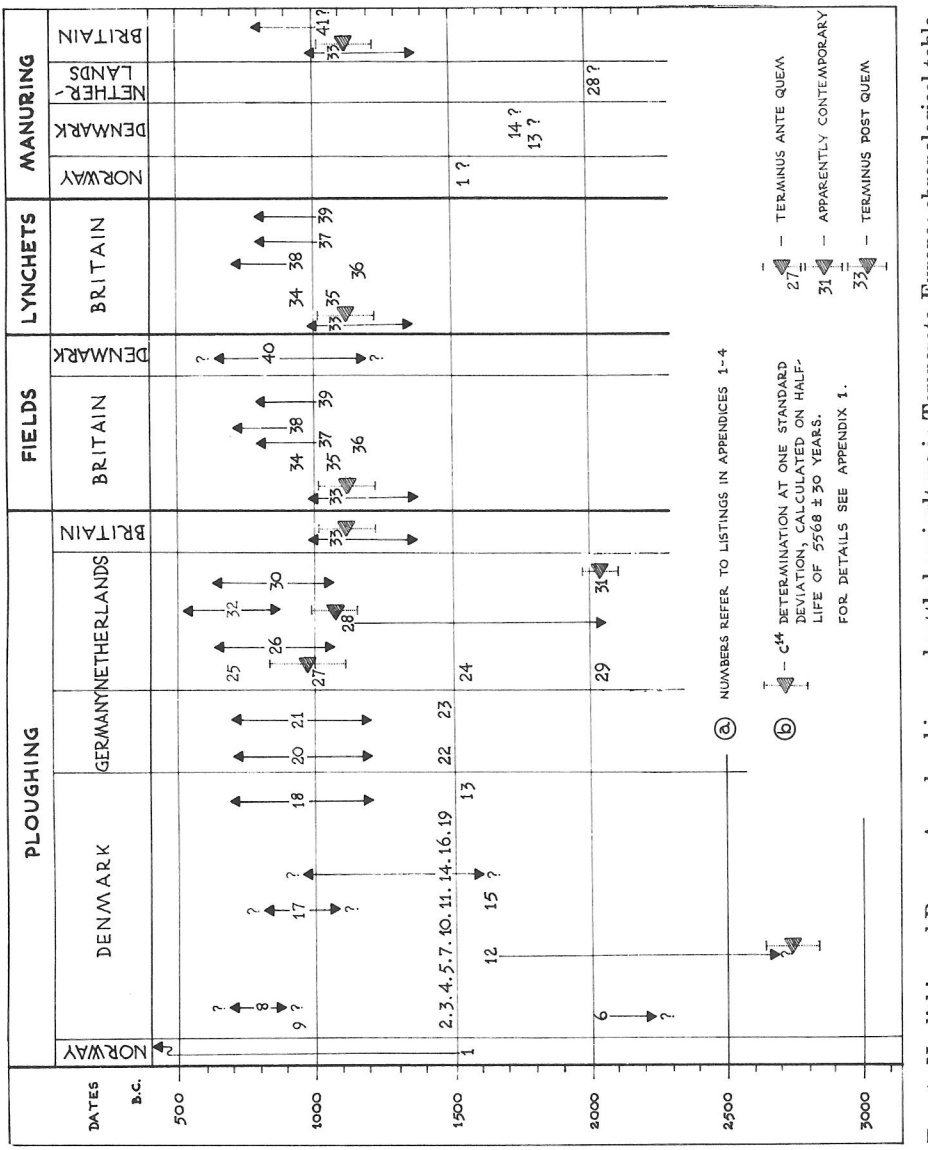

Fig. 1. Neolithic and Bronze Age ploughing and settled agriculture in Temperate Europe: chronological table.

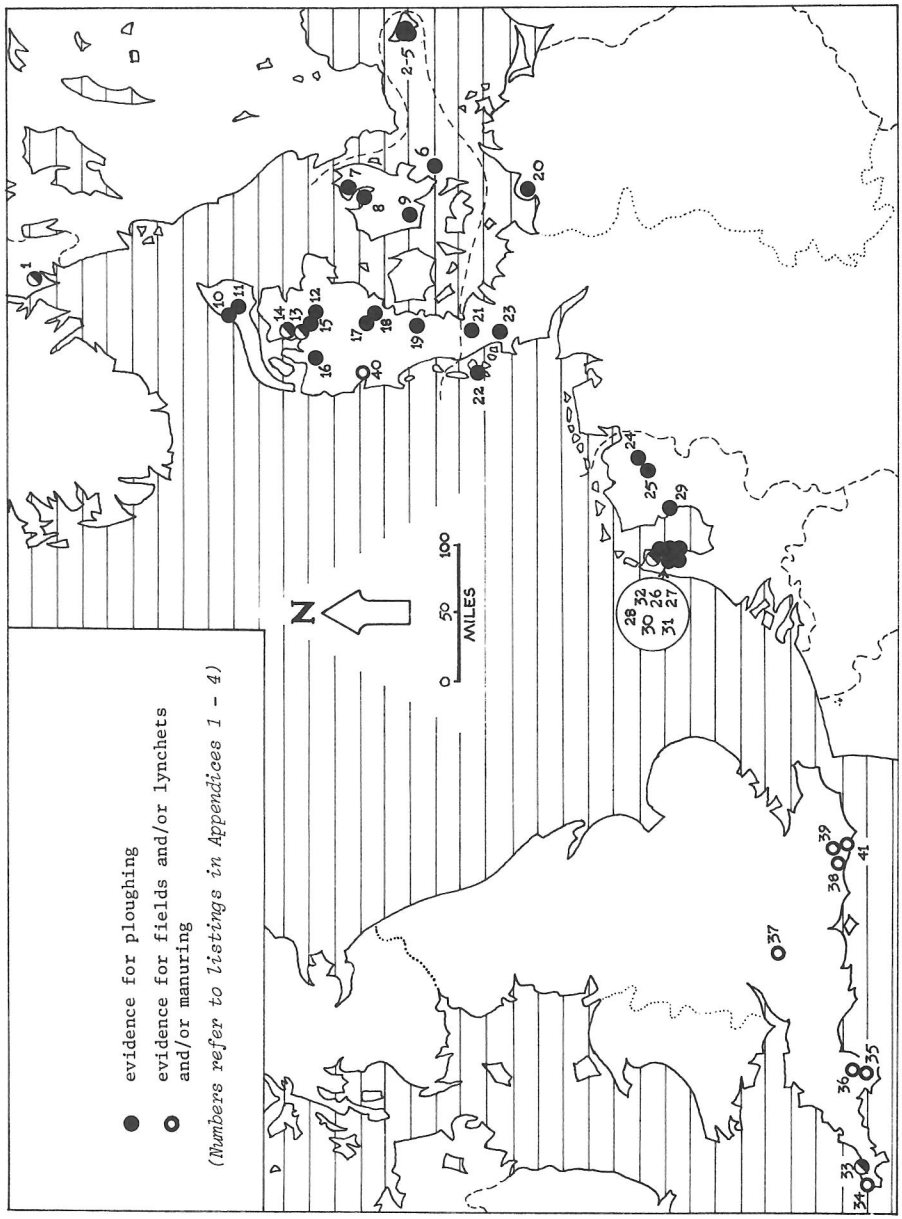

FIG. 2. Settled agriculture and ploughing distribution in Temperate Europe:Neolithic and Bronze Age.

that the ard is capable only of scratching and pulverizing. The general notion that the ard is suitable only for shallow cultivation may be incorrect also: at Lerchenfeldt (No. 11) Seeberg and Kristensen (1964) noted that in one place the furrows could be traced in section up through the old topsoil, showing that the depth of ploughing had been 20–25 cm. Nor was the ard necessarily used only on 'light' soils, since there is some slight evidence of its use on clays by the Late Neolithic (see Section VIII).

IV. Fields

A 'field' in this context should be taken to mean, initially, a plot of land contained by discernible boundaries. Such structures need not be arable (they could be animal pens, etc.), so I have deliberately excluded all enclosures of irregular form on the grounds that small ploughed fields are most economically of square or rectangular shape, particularly when adjoining to form a 'field-system'.[4] The question of lynchetted enclosures of irregular form is discussed in Section V. In short, by 'field' I refer here to the type of structure misleadingly called a 'Celtic' field.

If we may allow, then, that a 'field' is at least strongly suggestive of agriculture, its deliberately enclosed construction would argue for a degree of permanence. This should not be taken to imply annual cultivation, of course, since few soils will permit this. By 'permanent' we should envisage a field being a permanent part of a permanent field system, employed in some form of fallowing or rotation system, with or without manuring (see Section VI).

A considerable number of prehistoric and early historic 'Celtic' field systems are known from Temperate Europe, but most of these, where datable, are from the Iron Age or later. Müller-Wille (1965) in his exhaustive survey of field systems in Jutland, north Germany, and the eastern Netherlands, cites no convincing example of a field earlier than the Iron Age, with the possible exception of Fasterkjaer (my No. 40). Clark (1952:99) says that 'a few occurrences on the Sussex Downs (southern Britain) have been referred to the closing stages of the Bronze Age in that area'. However, the excavation of part of a Middle Bronze Age field system at Gwithian (Appendix 2, No. 33), and the revision of some parts of the British Middle/Late Bronze Age chronology (see Appendix 5), have placed a number of 'Celtic' field systems in southern Britain arguably into the Middle Bronze Age. In absolute chronological terms, they may date as early as the latter part of the second millenium B.C.

It must be emphasized that the preservation of ancient field systems is hazardous (see Bowen 1961: 1) due to later agricultural and industrial depredations, and that their dating is often extremely difficult. An examination of Appendix 2 will show most of the possible dating methods, summarized below:

(a) association with a site (e.g. settlement) of known date, (b) terminus post quem and/or terminus ante quem (the latter generally more useful for our purposes): field dated by evident stratigraphical or structural rela-

tionship to an earlier or later datable event, (c) direct dating: association of known-age material with construction and/or use of field. Fasterkjaer (No. 40) is tentatively dated to the Bronze Age by the sherds incorporated in the piles of stones cleared off the fields immediately prior to cultivation. At Gwithian (No. 33), besides dating by method (b) above, the consistent scatter of Middle Bronze Age material throughout the plough soil would itself strongly commend a Middle Bronze Age date for the cultivation of the fields.

V. Lynchets

Lynchets afford even more secure evidence for continuous cultivation of a field since, by current archaeological definition, they are the product of just such activity. Positive lynchets may have a 'core' of stone or turf, such as at Fyfield Down (Wiltshire, Britain) where the excavators suggest that stones cleared from the surface of the selected area were used to demarcate the field before cultivation started, the little 'wall' on the downhill side of the field thus becoming incorporated gradually into the positive lynchet (Bowen and Fowler 1962:105). But such instances clearly do not detract from the notion that lynchets are primarily caused by continued cultivation.

It is worth noting, however, that apparent 'lynchets' MAY form by natural soil-creep downhill, not accelerated by cultivation, where some linear impediment exists across the line of the slope—Macnab (1965:Pl. LIXa) shows a small positive 'lynchet' formed along the line of a post-and-wire fence at the bottom of a fairly steep slope. Therefore, I exclude the Dartmoor 'pound' sites (e.g. Radford 1952:61) from my list of true lynchet sites, since these are irregular enclosures, and the apparent 'lynchet' formation on the downhill sides may be due simply to soil accumulation against the stone walls or banks that delimit these 'pounds', unassisted by any cultivation at all.

Adequate excavation of lynchets has been extremely limited to date, so it is unsafe to appear definitive. My inclination, supported by my knowledge of such detailed observation (including excavation) as there has been, is to regard substantial lynchet formation on approximately square or rectangular fields as being the product of continuous cultivation, but to reserve judgment on (and therefore exclude from this paper) apparent lynchet formation associated with markedly irregular fields or enclosures. Bowen (1961:15–19) provides further consideration on lynchet formation.

Lynchets, in the sense defined above, are associated normally with evident field systems, but the reverse is not invariably the case. Thus the 'lynchet' sites listed in Appendix 3 all occur as 'field' sites in Appendix 2, although one site (No. 40) in the latter list does not occur in the former (though note that this due to the lack of ascertainable evidence in the literature: I have not visited this site personally).

Needless to say, the observations concerning the preservation and dating of fields (Section 4) apply also to lynchets.

VI. Manuring

The practice of manuring should be excellent evidence in favor of settled agriculture, since it is the lack of rapid soil regeneration that enforces shifting cultivation on most nonmanuring farmers. To establish manuring as a prehistoric cultivation method, however, may prove exceedingly difficult. Effective manuring, for instance, can be carried out by pasturing animals on stubble and fallow fields: such a practice MIGHT be suggested by careful soil analyses, but I know of no such demonstration.

The presence of domestic refuse in plough soil is commonly taken to indicate manuring, the potsherds etc. being transported to the field from the farm dung-heap to be distributed at random with the manure in which they are incorporated (Rhodes 1951:12–13; Appelbaum 1954:105; Megaw et al. 1961:210). The discovery of just such material in the plough soil of the Middle Bronze Age field system at Gwithian (Appendix 4, No. 33) was therefore interpreted as evidence for manuring (Megaw et al. 1961: 209–10). Burstow and Holleyman (1957:199–200) argue ingeniously, and to my mind plausibly, that the inhabitants of the Itford Hill site (Appendix 4, No. 41) practiced manuring—an argument on such negative evidence must of course depend upon more or less complete excavation of a settlement site, however, and this is not a common occurrence.

Nos. 1, 13, 14 and 28 of Appendix 4 do not show very substantial evidence for manuring, since the area of plough soil excavated was limited in each case. However, I have included these sites in order to show that there are other arguable examples of pre-Iron Age manuring besides Gwithian (No. 33).

VII. Paleobotany

Recently, Godwin has been able to demonstrate extensive and permanent prehistoric forest clearance in southeastern England (Godwin and Willis 1960:68–9). The Frogholt (nr. Folkestone, Kent) series shows widespread clearance and extensive agriculture from about the beginning of the Iron Age (Q-348 540 ± 130 B.C. [615 ± 134 B.C.]), but evidence for 'considerable agriculture' occurs at lower levels (Q-349 690 ± 110 B.C. (769 ± 113 B.C.) and Q-354 1030 ± 130 B.C. [1119 ± 134 B.C.]). At Wingham (Kent) radiocarbon dates Q-106 and Q-110 give, respectively, 390 ± 130 B.C. (460 ± 134 B.C.) and 1155 ± 110 B.C. (1248 ± 113 B.C.), and Godwin comments 'the two radiocarbon dates give a basis for the chronology of the whole deposit; extrapolation of this with pollen-analytic results suggests that from about 1600 B.C. to Roman time the Chalk here was open disforested country with arable land and pasture'. In other words, the Frogholt and Wingham series suggest settled agriculture from the late second millennium B.C. onward, with evidence that permanent clearance began appreciably before that time—say about the middle of the second millennium B.C.

Although the area of Frogholt and Wingham lies fifty-odd miles east of New Barn Down (No. 38), Plumpton Plain (No. 39) and Itford Hill (No.

41), these three sites are all on chalk downland, all may date as early as the later second millennium B.C. (see Appendix 5), and all show evidence for settled agriculture (see Appendices 2 and 3 for Nos. 38 and 39; Appendix 4 for No. 41). So it does not seem too far-fetched to draw attention to this as an interesting coincidence of archaeological and paleobotanical evidence.

VIII. Discussion

The evidence summarized briefly in Sections II–VII, and listed in Appendices 1–4, shows that we now have substantial evidence for plough agriculture over a wide area of Temperate Europe by the middle of the second millennium B.C. at the latest. A scatter of earlier datable sites (Nos. 6, 29, 31) suggests that plough agriculture is of considerably greater antiquity. It must be borne in mind that the majority of sites provide poor dating evidence: many of the barrows can be assigned a date in general terms only, by analogy of construction and burial rite to better-dated examples, and so forth. Thus an attribution of 'Bronze Age' to a barrow is not uncommon; greater precision is often not possible. Furthermore, in most instances the plough furrows are known merely to be 'earlier' than the barrow covering them. How much earlier we usually do not know. If Patzold (1960) is right (see Section III) that ploughing under barrows is a part of the burial ritual, then the date assigned to the primary burial of the barrow would apply to the ploughing underneath. But in most cases at least we cannot be certain that he is right.

Evidence for settled agriculture, in the form of fields, lynchets, or manuring, is more difficult to come by. As a rule, ancient field systems survive only in agriculturally marginal areas and often cannot be satisfactorily dated when they do survive. Nevertheless, despite such problems, we can see that there is substantial evidence from Britain of settled farming by the Middle Bronze Age, well before the end of the second millenium B.C. It is conceivable that settled farming in Temperate Europe was developed first in Britain, but there do not seem to be any very cogent reasons for supporting such an hypothesis, in view of the extremely slight chronological evidence currently available (see Fig. 1). I prefer to suggest that settled agriculture was widespread in Temperate Europe at this time, but remains to be satisfactorily documented.

It is unfortunate that the association of plough furrows with fields is so rare—only Gwithian (No. 33) can be dated with certainty earlier than the Iron Age. But this need not be taken to suggest that the two are usually, or frequently, unassociated, for normally they survive under different situations (see Sections II and III for discussion). Seeberg and Kristensen (1964:14) suggest that, under normal 'open' conditions (i.e. not covered by a barrow, or by some other rapidly accumulated level), the survival time of plough furrows might be quite short. Even if no subsequent cultivation took place, and there was no substantial erosion, there are various factors that would probably largely or wholly destroy such furrow traces—worm action, leaching, root penetration, etc.

Little attention has been paid to the evidence for manuring, and only Gwithian (No. 33) can be cited as strong evidence for this practice prior to the Iron Age. But the nature of the evidence, again, does not permit easy recognition (see discussion in Section VI), and this in combination with the lack of attention to the problem suggests to me that other equally early (and probably earlier) traces of manuring will eventually be recognized.

At present, then, it appears superficially as if plough agriculture was introduced into Temperate Europe substantially before settled agriculture. But, for reasons given, I am of the opinion that this may prove illusory.

A question often raised (e.g. Clark 1952:98) is: when did shifting 'slash-and-burn' agriculture give way to settled agriculture as the 'general' mode of cultivation? It is frequently suggested that a phase with emphasis on pastoral stock-raising intervened between the two, yet there is evidence (Clark 1952:98) that agriculture was practiced during the later neolithic, even without the evidence for ploughing during that period which we have discussed in Section III. Part of the answer may lie in the fact that, as already mentioned above, field systems normally survive only on 'marginal' lands. This has often been equated with the use of the ard to suggest that the agricultural technology could only exploit soils sufficiently light to be considered now as 'marginal'. But the argument clearly may be fallacious (e.g. Clark 1952:Fig. 46 demonstrates this very point for the pre-Roman Iron Age in Denmark).

Where data is available it does indeed appear that neolithic and Bronze Age ploughing and field systems are usually on light soils, often sandy. But at Aptrup (No. 12) the subsoil was hard clay (Seeberg and Kristensen 1964:14), and at Aldrupgaarde (No. 15) Kjaerum (1954:28) points out that the sandy subsoil below the barrow changes to clay only a few yards from the site, suggesting that clay as well as sand subsoil was probably cultivated there. Both of these sites are Late Neolithic, therefore by no means among the later pre-Iron Age examples of plough agriculture.

I am tempted to suggest, therefore, that soils other than 'light'/'marginal' soils might well have come under cultivation before the end of the Neolithic, at least by plough agriculture if not necessarily by settled agriculture. The majority of examples now known to us are those preserved by favorable conditions on 'light' soils, but these may have been 'marginal' even in the Late Neolithic. The paucity of Late Neolithic and Bronze Age settlements might possibly be used similarly: if settlement had by this time moved primarily onto heavier soils, then most of them would have been almost totally obliterated by subsequent continuous cultivation, and only the handful on 'marginal' lighter soils survived. At this stage the 'marginal' soils might have been used mainly for grazing. Some of the linear earthworks on the chalk downlands of southern Britain are attributed to the Late Bronze Age, though some at least may now be considered as probably Middle Bronze Age (see Appendix 5)—see Bowen (1961:35). The proposition that these earthworks demarcated 'ranches' might add something to the suggestion (above) of using marginal lands mainly for grazing by this

period, although not earlier on current evidence. The apparently substantial increase in cultivation of lighter soils during the Iron Age might then be interpreted not as the large-scale INTRODUCTION of settled agriculture, but as a large-scale EXPANSION of settled agriculture. Finally we should note, in any attempt to make or utilize broad classifications of this nature, that soils cannot be readily lumped into 'light' and 'heavy'. There are in fact an almost infinite number of gradations from one to the other (cf. Wooldridge 1956:225). Indeed, soil scientists usually classify soils on a three-way scale, not a two-way scale (cf. Bunting 1965:Fig. 5). This will have to be taken into account, together with the knowledge that soils change through time both by natural processes and by human use (Bunting 1965:19).

In conclusion, we have the problem of trying to associate plough agriculture and settled agriculture with defined cultural groups. As we have seen, the nature of the evidence makes this often a difficult question. To my mind it is hazardous to place any great weight on distributional correlations in view of the geographical factors affecting preservation and the differences in both archaeological skills and interests from country to country. And in view of the chronological problems involved (see above, frequently reiterated) it seems very difficult to make any attempt at present to assign the earliest known plough agriculture, or settled agriculture, to any particular cultural group. A particular factor to bear in mind is the virtual lack of evidence relating to the topics under discussion from the southern parts of Temperate Europe, since it is quite possible (if not probable) that both plough and settled agriculture were disseminated to Temperate Europe ultimately from western Asia and the lands of the Mediterranean. If we are to accept that the 'Battle-Axe/Single-Grave' complex is most likely to represent the material culture of the proto-Indo-Europeans, we might conclude, in the context of this volume, by considering them in relation to our evidence. The Late Neolithic barrows covering plough furrows appear to be generally 'Single-Grave' in archaeological nomenclature (Appendix 1 Nos. 12, 13, 24), and No. 14 shows cord-decorated pottery (i.e. of 'Single-Grave' type) in the plough soil, while No. 18 shows ploughing overlying five 'Single-Grave' burials. This is certainly not massive evidence to demonstrate plough agriculture by 'Battle-Axe/Single-Grave' peoples, but it is certainly not inconsistent with such an idea, as Seeberg and Kristensen (1964:14) point out. The examples cited, however, are not demonstrably early in the 'Single-Grave' sequence. It seems entirely plausible to suggest that at least the later 'Battle-Axe/Single-Grave' peoples (or some of them) practiced plough agriculture, though we cannot equate this, on present evidence, with settled agriculture. With regard to settled agriculture we can only say at present that the Middle Bronze Age (or M.B.A./L.B.A.) 'Deverel-Rimbury' people(s) of southern Britain practiced permanent plough agriculture, with manuring, of 'Celtic' fields by the later second millennium B.C.

ADDENDUM (JUNE 1967): Recent excavations at the South Street long barrow, Avebury, Wiltshire (England) have shown cross-ploughing underneath this barrow. Associated with the ploughing are a possible field boundary of stake holes, and probable slight lynchetting. No dates are

available as yet, but the barrow is presumably neolithic. Stratigraphically later cross-ploughing at the same site is associated with Beaker sherds. I am grateful to P. J. Fowler for drawing my attention to this and for his permission to include this information here. It is taken from a portion of the typescript of an article (Plough marks, lynchets, and early fields) by P. J. Fowler and J. G. Evans (the excavator of the Avebury long barrow mentioned above). This article was published in *Antiquity* in P. J. Fowler and J. G. Evans: "Plough-marks, lynchets and early fields", *Antiquity* 41 (1967) 289–301.

Appendix 1: Sites Showing Evidence for Pre-Iron Age Ploughing

NOTE: Müller-Wille numbers cited refer to his list of sites showing plough furrows (134–9; Figs. 32, 33). The same for Patzold.

NORWAY

Østfold

1. HUNN, barrow 38 Borge	Cross-ploughing below Roman Iron Age barrow. Ploughing dated to Late Neolothic on basis of flints in plough soil.	Herteig Müller-Wille No. 57

DENMARK

Bornholm

2. ALHØJ Pedersker	Cross-ploughing under Early Bronze Age barrow.	Patzold 227; No. 31 Müller-Wille No. 50
3. BILLE-GRAVSHØJ Pedersker	Cross-ploughing under Early Bronze Age barrow.	Patzold 227; No. 32 Müller-Wille No. 51
4. ST. LOFTS-GAARD Pedersker	Cross-ploughing under Early Bronze Age barrow.	Patzold 227; No. 33 Müller-Wille No. 52
5. JOMSFRU-GAARD Poulsker	Cross-ploughing under Early Bronze Age barrow.	Patzold 227; No. 34 Müller-Wille No. 53

Møn

6. LUNDEHØJ Praesto	Cross-ploughing under megalithic tomb containing TRB pottery, phase II–IV of Becker's TRB (Müller-Wille, 138). Does this mean B–C of TRB, or II–IV of the Danish Middle Neolithic?-B.W.	Patzold 224–7; Fig. 33; no. 30 Müller-Wille No. 49

Fredriksborg (Zealand)		
7. ASKEBAKKE-GAARD Torup	Cross-ploughing under Early Bronze Age barrow.	Ørnes-Christensen and Figs. 14, 15 Patzold 223–4; Fig. 31; No. 29 Müller-Wille No. 48 Lomborg
8. BALLERMOSEN Jagerpris	One-way ploughing, apparently: under Bronze Age barrow. This seems the only example of one-way (as opposed to cross-ploughing) ploughing in Temperate Europe prior to the Iron Age.	Patzold 222–3; Fig. 30; No. 28 Müller-Wille 114n.71
Sorø (Zealand)		
9. SLOTS BJAERGBY Slagelse	Cross-ploughing under Late Bronze Age (Montelius IV) barrow.	Glob 1947 Glob 1951 81 Patzold 220; Fig. No. 26 Müller-Wille No. 46
Hjørring (Jutland)		
10. GRØNHØJ Vendsyssel	Cross-ploughing under Early Bronze Age barrow.	Fries Patzold 219–20; No. 25 Müller-Wille No. 45
11. SONDER VRAA Vendsyssel	Cross-ploughing under ? Early Bronze Age barrow, but overlying under-graves.	Kunwald Patzold 218–9; Fig. 27; No. 24 Müller-Wille No. 44
Viborg (Jutland)		
12. APTRUP Sall, Houlbjerg	Cross-ploughing under barrow containing primary and secondary burials, both transitional bottom/ground graves. Ploughing overlies pit with C^{14} date K-955 2740 ± 100 B.C. (2881 ± 103 B.C.)	Seeberg and Kristensen
13. SOUTH BORUP Lastrup, Rinds	Cross-ploughing under Late Neolithic (ground grave) barrow. Five sherds of Neolithic type were found in and near the plough soil.	Seeberg and Kristensen

14. LERCHEN-FELDT Vesterbølle, Rinds	Cross-ploughing under a barrow which has ? Early Bronze Age primary burial, Late Bronze Age secondary burials. Cord-decorated (Late Neolithic) sherds in plough soil.	Seeberg and Kristensen
15. ALDRUP-GAARDE Hvorslev	Cross-ploughing under single grave (late under grave, or early ground grave) barrow.	Kjaerum Patzold 217; Fig. 25; No. 22 Müller-Wille No. 42
Ringkøbing (Jutland)		
16. SEVEL Sevel	Cross-ploughing under Early Bronze Age barrow.	Ørsnes-Christensen Patzold 217–8; Fig. 26; No. 23 Müller-Wille No. 41
Vejle (Jutland)		
17. VESTERLUND Vester	Cross-ploughing under Bronze Age barrow.	Hatt 1941 161–3; Figs. 8, 9 Hatt 1949 162, 173 Glob 1951 124; Fig. 105 Patzold 215–6; Fig. 24; No. 21 Müller-Wille No. 37
18. GADBJERG Gadbjerg	Cross-ploughing under Bronze Age barrow; ploughing overlies five flat graves of the Single Grave culture.	Müller-Wille No. 36
Haderslev (Jutland)		
19. SKRYDSTRUP Skrydstrup, Gram	Cross-ploughing under two Early Bronze Age barrows.	Becker Patzold 214; nos. 19, 20 Müller-Wille no. 33
GERMANY		
Mecklenberg		
20. WENDELSTORF Bad Doberan	Cross-ploughing under Bronze Age barrow.	Just 1957 Just 1958 Patzold 209–11; Fig. 21; No. 14 Müller-Wille No. 29

Schleswig-Holstein		
21. HARRISLEE a & b Flensburg	Cross-ploughing under two Bronze Age barrows.	Patzold 213–4; Nos. 17, 18 Roschmann 296 Müller-Wille No. 32
22. NEBEL barrow 236 Amrum Island	Cross-ploughing under Early Bronze Age barrow.	Kersten 1954 Kersten 1955 Kersten and La Baume Patzold 212–3; Fig. 22; No. 16 Müller-Wille No. 31
23. OSTENFELDE Husum	Cross-ploughing under Early Bronze Age barrow. Ploughing overlies graves with TRB pottery.	Patzold 211; No. 15 Müller-Wille No. 30
NETHERLANDS		
Drenthe		
24. GASTEREN Anloo	Cross-ploughing under Late Neolithic barrow.	van Giffen 1941 Fig. 31 van Giffen 1944 132 Hatt 1949 167, 173 Glob 1951 123–4 Patzold 201–2; Fig. 12; No. 3 Müller-Wille No. 21
25. HIJKEN Beilen	Cross-ploughing under Late Bronze Age barrow.	Müller-Wille No. 17
Noord-Holland		
26. ENKHUIZEN	Cross-ploughing under ? Bronze Age barrow.	Megaw et al. 211 Lehmann Müller-Wille No. 13
27. GROOTEBROEK I–III Grootebroek	Cross-ploughing under three Bronze Age barrows. Charcoal from primary burial of Barrow I gives C-14 date GrN-160 975 ± 140 B.C. (1004 ± 144 B.C.).	van Giffen 1953, 1954 Patzold 204–5; Figs. 15–17; Nos. 6, 7 Müller-Wille No. 14
28. OOSTWOUD Ostwoud	Cross-ploughing under two Bronze Age barrows. C^{14} date GrN-797 gives 1075 ± 80 B.C.) for one. The	Patzold 209; nos. 12, 13 Megaw et al. 213 van Giffen 1961 van Giffen 1961a

	plough soil contains fragments of Maritime Bell-Beaker and Veluwe beaker sherds.	van Giffen 1962 Müller-Wille no. 10
29. SCHOKLAND Noordostpolder	Cross-ploughing under level containing TRB and Veluwe beaker sherds.	Helinium III, 1963, 71 Müller-Wille no. 16
30. WERVERSHOOF Wervershoof	Cross-ploughing under ? Bronze Age barrow.	van der Waals Müller-Wille no. 12
31. ZANDWERVEN Opmeer	Cross-ploughing ? associated with level containing Vlaardingen and Beaker pottery. C^{14} date GrN-2221: 2040 ± 65 B.C. (2101 ± 67 B.C) for charcoal from ditch-filling associated with Vlaardingen pottery.	van Regteren Altena Müller-Wille no. 9
32. ZWAAGDIJK I and III Wervershoof	Cross-ploughing under two Late Bronze Age (?) barrows	van Giffen 1944 Patzold 208–9; figs. 19, 20; nos. 10, 11 Müller-Wille no. 11
BRITAIN		
Cornwall		
33. GWITHIAN Hayle	Extensive cross-ploughing at base and top of Middle Bronze Age plough soil. Level above contains material of late Middle Bronze Age. Pit immediately below plough soil produced charcoal to give C^{14} date NPL-21 1120 ± 103 B.C. (1212 ± 106 B.C.)(Callow et al.). Levels below plough soil contain Primary Neolithic, Beaker and Early Bronze Age materials.	Megaw et al. Müller-Wille no. 58

Appendix 2: Sites Showing Evidence for Pre-Iron Age Fields

BRITAIN

Cornwall

33. GWITHIAN Hayle	'Celtic' field system partially excavated, showing lynchets, stone banks, and a ditch bounding fields. See Appendix 1 for details.	Megaw et al. 207–9 Müller-Wille No. 58
34. TOWEDNACK St Ives	The Amalveor hoard of Middle or Late Bronze Age goldwork was found *dug into* (not *covered by*) a field bank. The hoard thus appears to constitute a terminus ante quem for this field.	Hawkes 1932 Hencken

Devon

35. FOALE'S ARRISHES Widecombe	Eight huts associated with field system recognizable despite later 'remodeling'. Radford Fig. 13 No. 6 is probably an Iron Age sherd (Fox 99), but the other pottery is classified as Late Bronze Age 'Deverel-Rimbury'. I would compare this pottery to ApSimon's (1958) Style IV of Cornish Bronze Age pottery, and to the pottery from Gwithian level 3 (Megaw et al. 202–4): both of these date to the later Middle Bronze Age. See Appendix 5.	T.D.A. 29 Radford 71–4, 77, 84; Fig. 8 No. 13; Pl. VIII Fox 90, 93, 99, 101; No. 23
36. SMALLACOMBE ROCKS Islington	Four huts associated with three fields. Pottery is called Late	T.D.A. 29, 157 Radford 73–4, 84; Fig. 13 Nos. 1–3; Pl.

	Bronze Age by Radford (74, 84) and Fox (99), but I would compare it to Gwithian level 5 (the plough soil) sherds (see Appendix 1 No. 33 for dating). See Appendix 5.	VIII Nos. 1, 2 Fox 93, 99, 101; No. 20
Wiltshire		
37. OGBOURNE MAIZEY Marlborough	This enclosure has not been excavated but has yielded 'Deverel-Rimbury' pottery from rabbit-scrapes, and is morphologically comparable to enclosures of Bronze Age construction. It is later than a field system. The date of the enclosure would now be considered probably Middle Bronze Age (see Appendix 5).	Piggott 1942 52; Fig. 3
Sussex		
38. NEW BARN DOWN Worthing	Field system linked to settlement site, which was called 'L.B.A. II', but would now be considered probably Middle Bronze Age (see Appendix 5).	Curwen and Curwen 32–5 Curwen 1934 137–70 Curwen 1938 43–4; Fig. 5 Clark 99, and n. 31
39. PLUMPTON PLAIN Lewes	Settlement site A is associated with a group of fields. The site was dated L.B.A. I. Site B, although close to fields, could not be associated directly with them. However, the molluscan evidence (Kennard) and the situation of Site B just above (i.e. uphill from) a positive lyn-	Holleyman and Curwen Hawkes 1935

	chet strongly suggest that Site B lies on a former field. Site B was dated 'L.B.A. II'. Both these sites would now be dated probably to the Middle Bronze Age (see Appendix 5).	

DENMARK

Ringkøbing (Jutland)

40. FASTERKJAER Faster	Müller-Wille (191) quotes Rosenberg (no ref. given) to the effect that Bronze Age potsherds were found in the stone clearance heaps of fields, suggesting a Bronze Age date for those fields.	Hatt 1949 15 Müller-Wille 191

NOTE TO APPENDIX 2

When this paper was presented in April 1966, the site of Ebbesbourne Wake (Elcombe Down, Wiltshire, Britain) was included as No. 37. A Late Bronze Age hoard was found here, buried just below the surface of a 'Celtic' field, indicating that cultivation had ceased by this time (Shortt; Bowen 1961 18 n.8, 37 n.2). However, Bowen (1962) reports that two sections were excavated across the lynchet on the lower side of this field: the only pottery found was of the Iron Age and Roman periods, suggesting cultivation then. Furthermore, Shortt (111–2) considers it possible that the bronze hoard might have been abandoned on this site by medieval or recent barrow looters.

The site has been withdrawn from Appendix 2, therefore, and subsequent site numbers adjusted accordingly. I am grateful to P. J. Fowler for drawing my attention to Bowen (1962).

Appendix 3: Sites Showing Evidence for Pre-Iron Age Lynchets

BRITAIN

Cornwall

33. GWITHIAN Hayle	Several lynchets noted during excavation, associated with field system. See Appendix 1 No. 33 for details.	Megaw et al. 209 Figs. 21, 24

34. TOWEDNACK St Ives	The eastern (downhill) boundary of this field is marked by a positive lynchet. See Appendix 2 No. 34.	
Devon		
35. FOALE'S ARRISHES Widecombe	The 'remodeling' of this 'Celtic' field system into a medieval strip-field system did not entirely obscure the lynchets of the earlier system. Details: see Appendix 2 No. 35.	Radford 71–3
36. SMALLACOMBE ROCKS Islington	These 'Celtic' fields have lynchets. Details: see Appendix 2 No. 36.	Radford 73
Wiltshire		
37. OGBOURNE MAIZEY Marlborough	'Celtic' field-system has lynchets. Details: see Appendix 2 No. 37.	Piggott 1942 52; Fig. 3
Sussex		
38. NEW BARN DOWN Worthing	'Celtic' field system has lynchets. Details: see Appendix 2 No. 37.	Curwen 1934 43–4; Fig. 5
39. PLUMPTON PLAIN Lewes	'Celtic' field system no. I has lynchets. Note that Site B is situated just above a positive lynchet, which must belong to an earlier field, perhaps contemporary with site A. Details: see Appendix 2 No. 39.	Holleyman and Curwen 18, 38

Appendix 4: Sites Showing Evidence for Pre-Iron Age Manuring

NORWAY

Østfold

1. HUNN Borge	The Late Neolithic flint material in the plough soil suggests manuring, but the area is too small to be more positive. Details: see Appendix 1 No. 1.

DENMARK		
Viborg (Jutland)		
13. SOUTH BORUP	Five sherds of Neolithic type from the plough soil suggest manuring, but the area is too small to be more positive. Details: see Appendix 1 No. 13.	
14. LERCHENFELDT Vesterbølle, Rinds	Two cord-decorated sherds in the plough soil suggest Late Neolithic manuring, but the area is too small to be more positive. Details: see Appendix 1 No. 14.	
NETHERLANDS		
Noord-Holland		
28. OOSTWOUD Oostwoud	Fragments of Maritime Bell-Beaker and Veluwe beaker from the plough soil suggest manuring, but the area is too small to be more positive. Details: see Appendix 1 No. 28.	
BRITAIN		
Cornwall		
33. GWITHIAN Hayle	The regular scatter of fragmented domestic refuse throughout the plough soil of fields 1, 2 and 3 is explained as the result of manuring. Details: see Appendix 1 No. 33.	Megaw et al. 209–10; Fig. 21
Sussex		
41. ITFORD HILL Newhaven	No fields or ploughing can be associated with this 'Late Bronze Age' (now probably Middle Bronze Age—see Appendix 5) site.	Burstow and Holleyman 199–200

But the absence of about 80 percent of the sherds from identifiable pots suggests that they were removed onto fields in the process of manuring.

Appendix 5: British Bronze Age Chronology

Although a full discussion of this topic cannot be attempted here, its relevance to the dating of some agricultural evidence under review here makes it necessary to provide a short summary. Briefly, the British Bronze Age (like much of the Bronze Age in other parts of Temperate Europe) has been peculiarly difficult to study due to (a) the paucity of settlement sites, (b) the scanty nature of grave goods other than pottery associated with burials, and (c) the usual occurrence of bronzes in 'hoards', not associated with either settlements or burials. Overall chronology has been built up in terms primarily of bronzes, and Abercromby's (1912) study of British Bronze Age pottery was necessarily effected largely *in vacuo*, so that his chronology was built essentially upon the basis of typological comparisons. For the pottery, one result was the gradual accretion of a vaguely-defined 'Deverel-Rimbury' culture (complex), based originally upon the finds from the Deverel barrow (Miles 1826) and the Rimbury cremation cemetery (Warne 1866), both in Dorset. By the early 1950's, the 'Deverel-Rimbury' complex was used to include a vast amount of material from southern Britain, considered to be generally of Late Bronze Age date.

In recent years, however, much 'Deverel-Rimbury' material has been shown to be of Middle Bronze Age date, rather than Late Bronze Age. The 'unity' of the complex has been broken both by chronological studies, and by detailed regional research, which now tends towards the establishment of regional 'cultures' (cf. Calkin's 'Cranborne Chase Culture' [1962]). Studies directly relevant to our purpose are outlined below.

1. Butler and Smith demonstrated in 1956 that (a) some bronze razors formerly considered Late Bronze Age (Piggott 1946) were, in fact, Early and Middle Bronze Age (though some might be as late as the Late Bronze Age) and (b) some types of pottery hitherto considered as 'Deverel-Rimbury' Late Bronze Age must be reassigned to begin in the earlier Middle Bronze Age, at the latest.

2. M. A. Smith's recognition of the 'ornament horizon' again showed that an earlier date than hitherto supposed for some 'Late Bronze Age' bronzes in southern Britain.

3. The discovery and excavation of three Bronze Age occupation sites in Cornwall during the 1950's bears directly upon this problem. Abercromby recognized that southwestern Britain, Cornwall particularly, maintained a distinctively 'regional' culture during much of the Bronze Age.

Patchett (1944, 1950) further studied the Cornish Bronze Age pottery, but still in the enforced context of burial associations, mostly without datable grave goods. She considered that the later pottery in her study was of 'Deverel-Rimbury' affinities, and must therefore derive from a Late Bronze Age intrusion into the southwest of 'Deverel-Rimbury' peoples from the Dorset/Hampshire area.

a. Trevisker (St Eval, Cornwall) showed a stratified sequence of pottery starting with later Early Bronze Age (roughly, Wessex II) and concluding in the later Middle Bronze Age. The excavation report is not yet published, but a brief analysis of the pottery is included in ApSimon (1958), and that article concludes with the suggestion that the globular urn element in the 'Deverel-Rimbury' complex is, in fact, the result of an expansion into the Wessex area of a Cornish Middle Bronze Age group with several centuries of regional development in the southwest behind them (ApSimon 1958:44).

b. The Bronze Age site at Gwithian (Hayle, Cornwall) is discussed in part elsewhere in this paper (and see Appendices 1–4, No. 33). The stratified sequence is generally similar to that at Trevisker, though it is clearer here that the pottery of 'Deverel-Rimbury' affinities really is Middle Bronze Age, and not later.

(c) Kynance (The Lizard, Cornwall): Thomas (1960) reported no stratigraphy within the Bronze Age occupation, but ApSimon (1962:319–20) and Wailes (1961) both consider the Kynance Bronze Age pottery to be generally similar to and contemporary with the sequences from Trevisker and Gwithian mentioned above. The Kynance Bronze Age level produced a mold for a flat bronze axe (Thomas 1957), again favoring an earlier dating.

ApSimon (1962) is an expansion of his suggestions put forward in ApSimon (1958), that the evidence from domestic Bronze Age sites in Cornwall demonstrates a regional continuity of ceramic tradition from the Early Bronze Age to at least the later Middle Bronze Age, and that the supposed 'Deverel-Rimbury' elements in the southwest are therefore to that extent indigenous. Thus his argument that there was a spread from Cornwall to Wessex during the Middle Bronze Age, rather than in the other direction during the Late Bronze Age.

The rapid and incomplete survey above summarizes the reasons for regarding at least a substantial part of the 'Deverel-Rimbury' complex[5] as Middle Bronze Age. Since most of the known British Bronze Age field systems are 'Deverel-Rimbury' in cultural affinity, the importance of this whole chronological question to this paper is obvious. Two radiocarbon dates from relevant sites show no conflict with this revised dating, although I would hesitate to attribute too much weight to this scanty evidence. These dates are:

NPL-19 1180 ± 180 B.C. (1274 ± 185 B.C.) from Shearplace Hill (Sydling St. Nicholas, Dorset), the 'Deverel-Rimbury' site whose pottery in assessed in ApSimon (1962). Excavation report: Rahtz (1962). Radiocarbon date: Callow et al. (1963).

NPL-21 1120 ± 103 B.C. (1212 ± 106 B.C.) from level 5 at Gwithian (Hayle, Cornwall). See Appendix 1 No. 33 for further details.

Acknowledgments

I would like to thank the participants of the Conference for their interest and comments. In particular I am grateful to Professors A. Tovar and Warren Cowgill who both supplied written comments, appended below. I am also most grateful to Mr. Peter J. Fowler and Dr. Ian H. Longworth, both of whom read this manuscript in its original form, and made helpful suggestions and criticisms: responsibility for any errors or ambiguities in this final version is, however, mine alone.

COMMENT 1: Professor Warren Cowgill

There is a root **arə-* 'to plow' attested in Celtic, Italic, Germanic, Balto-Slavic, and Greek. A common word for the 'plow' itself is attested in all these languages, plus Armenian. This word is **arə-tro-m*, formed with a regular Indo-European instrument suffix: *arətrom* is 'the thing with which one *arə*-s'. Tocharian *ārə* 'plow' is clearly from the same root, but appears to have a different suffix. The only major subgroups where this root does not occur are Albanian, Anatolian, and Indo-Iranian.

The immediate interpretation of this information would be that Indo-Europeans practiced plow agriculture at least as far back as the time when their proto-language was just breaking up; that is, around the middle of the third millennium B.C. It would suggest that already the earliest corded ware cultures of Europe should have used plows.

To be sure, it is thinkable that the plowing words could have spread along with the plow itself from one Indo-European group to another after their speech forms had begun to diverge but when they were still enough alike that the resulting outcomes in the attested languages look like common inheritance from the proto-language.

It is hard to estimate on purely linguistic grounds the latest date at which such borrowing could have occurred. The earliest fixed date is the occurrence in the Pylos tablets of *aroura* 'plowland', in which PIE **ə* had already become Greek *-o-*. But if the Greek word is the source of the 'plow' words in other IE languages, the borrowing must have taken place before **ə* had become Greek *o*, i.e. before at the latest 1200 B.C. But how much earlier I see absolutely no way of telling on purely linguistic evidence.

On historical and geographic evidence I would imagine though that the Indo-Europeans must have possessed plows well before 2000 B.C. One reason is that I doubt that both a root and a specific word **arə-tro-* would have diffused over so wide an area after that time. A more specific reason is that it seems unlikely that the Greeks, if they did not already have plow words when they came to Greece (ca. 2200 B.C.?), would have borrowed them from other Indo-Europeans to the north rather than from some language of Anatolia, Mesopotamia, or Greece itself.

The upshot is that the date you mention on page 286, 'by the middle of the second millennium B.C. at the latest', seems comfortably inside the date

that is suggested by linguistic-geographic evidence, and I would suspect that archaeological research may eventually push the date back to the end of the third millennium B.C.

COMMENT 2: Professor A. Tovar

My comments on Mr. Wailes' review of archaeological evidence must be in a different vein. Of course the relative lack of study on prehistoric agriculture in southern Europe is a factor which currently prevents archaeologists from establishing any real data on agricultural diffusion in that area.

A linguistic study would help to make more precise any conclusions about this diffusion of agriculture. But probably, without the control of archaeological research, what the linguists can say is still impressionistic and unsure. I think that the diffusion of some old agricultural terms, like 'corn' (which is found in Indo-European, but, it seems, also in Semitic and Hamitic, and in peripheral languages like Basque and some of Caucasia), would underline the idea of a cultural diffusion not restricted to just one of the elements of the Battle-Axe/Single Grave complex. I could refer to a chapter (in a book of mine: El euskera y sus parientes, Madrid 1959) on a king of ancient Spain, called Gargoris, who is the grandfather of a cultural hero, father of Agriculture, and whose name recalls the Basque words for 'barley' and 'wheat' (*garagar* and *gari*), with parallels in both Hamitic and Caucasian. The word may be identical with IE *corn*, *granum* etc. and speaks for a very old and general diffusion of agriculture.

Notes

[1] E.g. Bølling Sø (J. Iversen Land occupation in Denmark's Stone Age, *Danmark's Geologiske Undersøgelse*, II, Raekke, no. 66, Copenhagen 1941).

[2] See H. Godwin, Half-life of Radiocarbon, *Nature* 195 no. 4845, 8 September 1962, 984.

[3] Some of the ploughing-scenes depicted in rock-carvings in the Val Camonica are arguably of Early and Middle Bronze Age date (E. Anati *Camonica Valley*, 1964, 43ff.), but this area does not fall within the usual definition of 'Temperate Europe'.

[4] Thus, for example, I have deliberately excluded the 'fields' on Shetland reported in C. S. T. Calder: Report on the discovery of numerous Stone Age house-sites in Shetland, *Proc. Soc. Antiquaries of Scotland* 84, 1955–6, 340–97. These enclosures are probably Neolithic, but their irregular shape (as illustrated in the report cited) does not suggest that they were ploughed; they might have been cultivated with spade and/or hoe, but might also have been stock-enclosures.

[5] Despite general recognition of its compound nature, this term is still retained by most workers as a convenient label, at least until re-analysis is further advanced.

[6] This paper was read in April 1965, and the final revised manuscript completed in spring 1967. Further evidence discovered and/or published since then has not been incorporated, therefore.

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Abbreviations

Aarb.	Aarbøger for nordiske oldkyndighed og historie
Ant.	Antiquity
Arch. J.	Archaeological Journal

P.P.S. Proceedings of the Prehistoric Society
P.W.C.F.C. Proceedings of the West Cornwall Field Club
S.A.C. Sussex Archaeological Collections
T.D.A. Transactions of the Devon Association
W.A.M. Wiltshire Archaeological and Natural History Magazine
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Les valeurs économiques dans le vocabulaire indo-européen

Émile Benveniste

Dans le vocabulaire de l'économie indo-européenne, qui est une économie pastorale, il y a un terme d'importance capitale, **peku-*, attesté sur trois grandes aires dialectales: indo-iranien, italique, germanique.[1]

Tous les comparatistes s'accordent à voir dans **peku-* le nom indo-européen du 'bétail' et à l'expliquer par une racine **pek-* 'tondre'. Ce terme désignerait donc proprement l' 'ovin' en tant que porteur d'une toison, et il aurait été généralisé pour l'ensemble du 'bétail'.

Nous essayons de montrer ici que cette conception de **peku* est insoutenable et qu'il faut reprendre l'analyse des données. L'examen portera successivement sur l'indo-iranien, le latin, et le germanique, et conduira à des conclusions qui dépassent le problème considéré.

I. Indo-Iranien

Les formes de véd. *paśu* et d'av. *pasu-* ont été enregistrées depuis longtemps et, aisément accessibles chez Grassmann et chez Bartholomae, elles ne seront pas recensées ici.

En védique, dans l'ensemble, le sens est celui de 'bétail', confirmé par diverses circonstances de l'emploi: liaison avec *vraja* 'étable', avec *gopā* 'berger', avec *yūtha* 'troupeau', etc. . . . Il y a cependant à observer:

1. Que *paśu* est un terme collectif couvrant toutes les espèces d'animaux domestiques et seulement celles-là: *áśvāvantaṃ gómantam . . . paśúm* (I 83, 4); *paśúm áśvyam gávyam* (V 61, 5); etc . .

2. Que *paśu* englobe même l'homme désigné comme *paśu* bipède, sur la même ligne que le *paśu* quadrupède: *dvipáde cátuṣpade ca paśáve* (III 62, 14). Le rapprochement classique avec l'ombrien *dupursus peturpursus* est, dans le cas présent, inopérant:[2] l'expression ombrienne (VI b 10–11) remise dans son contexte *tote iouine, ocrer fisie, totar iouinar dupursus peturpursus . . .* 'urbi Iguvinae, arcis Fisiae urbis Iguvinae bipedibus quadrupedibus . . .' désigne par 'bipèdes et quadrupèdes' l'ensemble des êtres vivants. Mais le texte védique applique cette distinction au *paśu*, ce qui est tout différent. L'homme est inclus dans la notion de *paśu*. Ce n'est pas seulement de ce passage qu'on peut l'induire: c'est l'enseignement explicite du ŚB. VI, 2, 1, 2 sur les cinq *paśu*: *pañca paśūn*: *puruṣam aśvaṃ gām avim ajam* 'homme,

cheval, boeuf, brebis, bouc' et d'autres textes qui transposent cette définition dans la théorie du sacrifice.

L'inclusion de l'homme dans le *paśu* est à interpréter comme l'indice d'une forme de société pastorale où la richesse mobilière se composait à la fois d'hommes et d'animaux, et où le terme *paśu*, signifiant d'abord cette richesse mobilière, pouvait convenir aux bipèdes comme aux quadrupèdes.

Nous pourrons alors remarquer que cette association des hommes et des animaux, implicite dans la définition védique, est explicitée par la formule avestique *pasu vīra*, dvandva dont l'antiquité est reconnue depuis longtemps.

Que désigne au juste *vīra* 'homme' dans le dvandva avestique *pasu vīra* auquel fait écho, à l'autre bout de l'aire indo-européenne, le *uiro pequo* des Tables Eugubines? On a beaucoup discuté sur le sens exact de **wiro-*: était-ce le 'mâle' ? le 'guerrier' ? Ni l'un ni l'autre ne s'applique ici. Le problème a été mal posé, comme tous les problèmes qu'on débat hors de leurs conditions propres. Pour établir le sens de **wīro-* il faut voir à quoi il se lie et a quoi il s'oppose. Or, pour le sanskrit, Lüders a montré que *vīra*, dans un contexte où il est lié à la notion de bétail, désigne 'l'esclave'.[3] Ce sens, qu'on le prenne strictement ou qu'on l'atténue en 'gens de maison, domestique', vaut aussi pour le *vīra* avestique dans *pasu vīra*.

Nous en donnerons une nouvelle confirmation, que nous tirons d'une gāthā de Zarathuštra. Dans une strophe d'accent pathétique (Y.46.2), Zarathuštra se plaint de son impuissance à vaincre l'hostilité qui l'entoure de toutes parts: *vaēdā taṯ yā ahmī mazdā anaēšō mā kamnafšvā hyatčā kamnānā ahmī* 'je sais pourquoi je suis sans pouvoir, ô Mazda; c'est que je suis *kamna-fšu* (= j'ai peu de *pasu*) et que je suis *kamna-nar-* (= j'ai peu d'hommes).' Les deux qualifications *kamna-fšu* 'qui a peu de *pasu*' et *kamna-nar-* 'qui a peu d'hommes' procèdent évidemment du dvandva *pasu vīra* avec une transposition de *vīra* en *nar* connue dans l'Avesta aussi (Y.4,5; Vd. 13, 31–32). C'est d'être pauvre en *pasu* et pauvre en *nar-* qui rend Zarathuštra 'impuissant, *anaēša*'. Les deux possessions ensemble confèrent la puissance. Ce sont les deux espèces de la richesse mobilière.

Nous ajouterons donc la locution gāthique *kamnafšu- kamnanar-* au répertoire avestique des composés bâtis sur le dvandva *pasu vīra* et qui procèdent par termes couplés.

drvafšu- drvō.vīra (Y. 16, 10, *cf.* Y. 58, 6)
varətafšu- varətō.vīra (Vd. 8, 4)
frādaṯ.fšu- frādaṯ-vīra (Y. 2, 4–5)
frafšu- fravīratā (Visp. 12, 1)

En dehors de cette locution formulaire, *pasu-* et *fšu-* peuvent se présenter dans les mêmes composés: comparer *haurvafšu-* et *pasuš.haurva-* 'gardien du *pasu*'; *drvō.pasu-* et *drvafšu-*.

Plus techniques et formés au stade où *pasu-* (*fšu*) désignait étroitement le bétail sont les composés: *pasuš.x^{v}arəθa* 'nourriture du *pasu*'; *pasu-maza-* '(contrat) garanti par une tête de bétail'; *pasu-vastra-* 'vêtement de (peau de) mouton'; *pasuš-hasta-* 'étable'; *maṯ.fšu-* 'avec le bétail'.

Le 'possesseur de pasu' (*fšumant-, fšu-šan*, cf. véd. *paśumant, kṣumant-, paśuṣan-*) est grandement loué; une eulogie particulière, dite *fšušō mąθrō* 'formule rituelle du fšušan' (Y. 58, 4–7) lui est consacrée.

Sous toutes ces formes, grâce à la diversité des témoignages linguistiques, on aperçoit l'importance du *pasu* dans la société pastorale du Nord-Est de l'Iran, dont l'idéologie marque les portions les plus anciennes de l'Avesta.

Nous nous bonons à la phase ancienne, sans suivre l'histoire ultérieure, d'ailleurs connue, de *pasu-*, qui est devenu aujourd'hui le nom du 'mouton' dans une partie de l'iranien (sogd. *ps-*, oss. *fus* etc.). Cette nouvelle spécialisation succède à celle qui a, dans une période bien plus ancienne, conféré à *pasu-* le sens de 'bétail'.

Au total, c'est comme élément de la richesse mobilière qu'il faut prendre le *vīra* avestique dans *pasu vīra*. On désigne par cette locution l'ensemble de la possession mobilière privée, autant les hommes que les animaux, l'homme étant tantôt englobé dans le *pasu*, (*fšu*), tantôt mentionné séparément.

On peut étendre la même interprétation au *uiro* ombrien, non pas seulement parce que la formule *uiro pequo* vient d'un héritage indo-européen commun, mais en vertu d'un indice spécifique, propre à deux peuples italiques: les Ombriens et les Latins. On n'a pas encore prêté attention à une similitude frappante entre le formulaire ombrien et un passage de la vieille prière que Caton cite. Il s'agit d'une expression rituelle répétée onze fois, en ombrien: *uiro pequo . . . salua seritu* 'salva servato' et chez Caton: *pastores pecuaque salua seruassis*. Il suffit de superposer les deux textes:

ombrien *uiro pequo . . . salua seritu*
latin *pastores pecuaque salua seruassis*

pour qu'apparaisse la correspondance étroite des formulaires. Tous les termes successifs se répondent dans l'identité étymologique, sauf le premier où le même sens est énoncé par des termes distincts: c'est justement ombrien *uiro*, dont l'équivalent latin est, non *viros*, mais *pastores*. D'où il résulte que l'ombrien désignait par *uiro*, lié à *pequo*, les hommes chargés des soins du bétail. Nous avons donc en ombrien le parallèle exact de la notion de *vīra* associé à *paśu* en indo-iranien.

Que *paśu* renvoie d'abord a une valeur économique, on peut en trouver confirmation dans le terme *kṣu* qui, quoiqu'apparenté à *paśu-* comme av. *fšu-* à *pasu-*, s'en est détaché de bonne heure et garde mieux le sens premier.

L'adjectif *purukṣú-* signifie 'abondant en richesses, en possessions', mais non spécifiquement en 'bétail'. C'est une qualification des dieux Agni, Indra, Soma. Plus instructives sont les liaisons formulaires: fréquemment avec *rayí-*, et en équivalence avec *vásumant-*, ainsi *rayím . . . vásumantaṃ purukṣúm* (VI, 68, 6); ou avec *puruvīra-* ou en outre *gómant-* et résumant en quelque sorte les diverses sortes de biens meubles: *gómantaṃ . . . suvīram rayím . . . purukṣúm* (IV, 34, 10) ou *rāyáḥ puruvīrasya nṛvátaḥ purukṣóḥ* (VI 22, 3).

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il doit se relier à un *adjectif* ancien et non au neutre historique *pecu*; ou *pecūnia* dérive directement du neutre *pecu*, mais par une suffixation qui n'est plus immédiatement comparable à celle des abstraits slaves en *-ynji*.

L'autre substantif dérivé de *pecu* est *pecūlium*. Ici encore il s'agit d'une forme qui reste isolée et sans analogue parmi les neutres en *-ium*. On peut néanmoins en démêler la formation. Entre *pecu* et *pecūlium*, on doit poser un intermédiaire **pecūlis* qui est à *pecu* comme *īdūlis* à *īdus*, *tribūlis* à *tribus*. Pour le rapport entre **pecūlis* et *pecūlium*, on comparera *edūlis* et *edūlia* (d'où *edūlium*). De *pecūlium* on tire un verbe dénominatif **pecūlo(r)*, d'où le substantif *pecūlātus -ūs*. Ainsi la série *pecūlium*: **pecūlo(r)*: *pecūlātus* devient parallèle à *dominium*: *dominor*: *dominatus*. Toute la chaîne des dérivés qui se groupent avec *pecūlium* s'organise ainsi rationnellement.

L'essentiel est maintenant le problème du sens de *pecūnia*, du sens de *pecūlium* et de leur relation avec *pecu*. Depuis qu'il y a des étymologistes, cette relation passe pour évidente: *pecu* est le 'bétail'; *pecūnia*, la 'richesse en bétail', *pecūlium*, la 'part de bétail laissée à l'esclave'. Tel est l'enseignement de tous les dictionnaires étymologiques et des ouvrages de morphologie latine.

Un examen plus attentif des données et des relations où ils s'ordonnent conduit à récuser l'interprétation traditionnelle, qu'on peut dire séculaire, et même millénaire, des trois termes *pecu*, *pecūnia*, *pecūlium*, qui nous vient en effet des étymologistes romains.

Il faut ici prévenir tout malentendu. La relation formelle entre ces trois termes est assurée. Il s'agit de savoir comment l'entendre, et d'abord d'établir ce que signifient *pecūnia* et *pecūlium*.

A. Pecūnia.

On peut consulter tous les auteurs de la latinité ancienne et classique, parcourir toutes les citations de dictionnaires; jamais on ne verra aucun rapport de fait entre *pecūnia* et *pecu*. Tous les exemples de *pecūnia* sans exception se rapportent au sens de 'fortune, argent', et se définissent par 'copia nummorum'. Nous avons examiné tous les passages de Plaute: *pecūnia* n'est jamais autre chose que l'argent, la fortune; *pecunia praesenti* 'au comptant', (*magna*) *pecunia emere*, locutions fréquentes, le confirment; de même chez Cicéron *pecunia numerata* 'argent comptant'; *pecunia publica* 'trésor public', *pecuniaria res* 'affaire d'argent', *pecuniosus* 'riche', etc. Bien mieux *pecūnia* était, dans l'usage classique, si différente de *pecus* qu'on pouvait au besoin employer les deux mots dans la même phrase sans pléonasme de sens ni faute de style: Liv. 29, 31 *familiae aliquot cum mapalibus pecoribusque suis* (*ea pecunia illis est*) *persecuti sunt regem* 'un certain nombre de familles numides, avec leurs cabanes et leurs troupeaux (c'est là toute leur fortune), suivirent le roi'. Il est clair que pour l'historien *pecora* et *pecūnia* sont des notions entièrement distinctes.

Il ne suffit pas d'avoir expliqué le lien formel qui rattache *pecūnia* à *pecu*. Il faut élucider parallèlement le rapport de sens qui résulte du rapport de dérivation. On doit ici procéder par inférence méthodique sans tenir compte

des interprétations traditionnelles. Si le dérivé *pecūnia* a exclusivement, des les premiers emplois, le sens d' 'argent, fortune, *khrḗmata*' on en conclura que le terme de base *peku* se rapporte exclusivement à une valeur économique et qu'il signifie 'possession mobilière'. Ainsi seulement sera justifié le sens constant de *pecūnia*: c'est un abstrait-collectif du type que nous rencontrerons dans gr. *próbasis*, et il généralise le sens propre à *pecu*.

C'est en vertu d'un procés distinct, tout pragmatique et secondaire, que **peku* dont le sens était 'possession mobilière' a été appliqué spécifiquement à la réalité dite 'bétail'. Il faut distinguer dans cette analyse les deux plans théoriques: celui de la signification et celui de la désignation. Il faut distinguer en conséquence le sens propre de **peku*, révélé par ses dérivés anciens, et l'emploi historique du mot *pecu* pour désigner le 'bétail'. Une fois réalisée la jonction entre *ce* terme **peku-* et *cette* réalité, le bétail, de nouveaux développements peuvent toujours se produire: c'est le cas, par example, des différenciations opérées en latin entre (1) *pecu*, (2) *pecus -oris*, (3) *pecus -udis*. Elles relèvent de l'histoire lexicale du latin et n'intéressent plus les relations fondamentales que nous mettons au jour.

Ce sont ces relations qui ont été méconnues. On les fausse depuis qu'on interprète inexactement tant *pecu* que *pecūnia*. Et ces notions inexactes ont été, d'abord par les Latins, puis par les modernes, transposées dans la traduction naïve de *pecunia* par 'richesse en bétail', que tout réfute. La nature réelle du *peku* primitif s'éclaire à partir du sens réel du *pecūnia* historique. Ovide même en donne à son insu la meilleure confirmation quand, raisonnant sur les mythes et les mots, il se fait enseigner par la déesse Flore l'origine des jeux:[5] *Dic, dea, ludorum . . . quae sit origo*. Et la déesse de répondre aussitôt:

Cetera luxuriae nondum instrumenta vigebant:| aut pecus aut latam dives habebat humum:| hinc etiam locuples, hinc ipsa pecunia dicta est.

'Les autres manifestations du luxe n'avaient pas cours encore. Le riche possédait ou un *pecus* ou un vaste domaine; de là vient qu'on dit *locuples*, et qu'on dit *pecunia*'.

Les notions se définissent ici clairement, leur nature ressort de leur opposition. Ovide pose les deux seules formes de richesse que la société connût alors: la richesse immobilière, représentée par la *lata humus*, dont le possesseur était dit *locuples*, et la richesse mobilière, représentée par le *pecus*, et énoncée par le terme *pecunia*. Mais le texte même laisse voir en outre l'écart survenu entre *pecus* qui, dans l'usage classique, a été spécialisé pour 'troupeau, bétail', et *pecunia* qui a gardé son sens générique de 'richesse mobilière'. Cette distance de sens est l'indice qui aide à restaurer une relation déviée.

La notion de 'richesse mobilière' exprimée par *pecūnia* pouvait englober d'autres espèces que le 'bétail'. On se fera une idée de son extension première par cette notice de P. Festus qui doit se référer à une locution archaïque:

pecunia sacrificium fieri dicebatur cum fruges fructusque offerebantur, quia ex his rebus constat quam nunc pecuniam dicimus.

Pour ce glossateur, les *fruges fructusque* constituaient la *pecunia*. On peut

enregistrer cette valeur élargie de *pecunia* sans rejeter, mais en les réinterprétant, les définitions de Varron: *pecuniosus a pecunia magna, pecunia a pecu: a pastoribus enim horum vocabularum origo.*[6]

Il suffit en effet de lire Varron même pour savoir ce qu'on entendait de son temps par *pecunia*. Il fait entrer, sous la dénomination de *pecunia*, des termes comme *dos* 'dot', *arrabo* 'arrhes', *merces* 'salaire', *corollarium* 'pourboire' (V 175); puis *multa* 'amende' (177); *sacramentum* 'dépôt sacré' (180); *tributum* 'tribut' (181); *sors* 'pecunia in faenore' (VI 65); *sponsio* 'dépôt garantissant une promesse de mariage' (VI 70). Il y avait en outre la *pecunia signata*, l'argent monnayé (V 169), les *nuncupatae pecuniae* des textes de lois (VI 60); bref, *pecūnia* couvre tous les usages possibles de l'argent comme valeur économique ou comme signe monétaire, mais encore une fois, il ne se réfère jamais à la possession du 'bétail'. Cela veut dire que, quand *pecu* s'est spécialisé dans la designation du 'bétail', il n'a pas entraîné *pecūnia*, qui a conservé la valeur ancienne de fortune mobilière.

B. Pecūlium

Ce qui a été dit de *pecūnia* vaut aussi, dans une assez large mesure, de *pecūlium*. Nous avons ici affairs à un terme qui, disons-le d'emblée, est encore plus loin de *pecu* que ne l'était *pecūnia*. On sait que *pecūlium* désigne la possession propre concédée à ceux qui ne peuvent légalement posséder, l'épargne personnelle accordée a l'esclave par le maître, au fils par le père. La notion d' 'avoir propre' est au premier plan,[7] et cet avoir consiste toujours en biens meubles: argent, ou moutons. Nous n'avons pas à nous demander pourquoi *pecūlium* se rapporte aux économies de l'esclave et *pecūnia* à la fortune du maître; c'est là un problème d'histoire des institutions, non de la forme linguistique. Cette répartition constatée, nous retrouvons le sens de *pecūlium* dans le dérivé *pecūliaris* 'propre au *peculium*' ou 'donné en *pecūlium*'. Un exemple intéressant de l'adjectif est chez Plaute *Asin.* 541: *Etiam opilio qui pascit . . . alienas oves, aliquam habet peculiarem, qui spem soletur suam* 'même le berger qui mène paître les brebis des autres, en a une à lui, qu'on lui laisse en propre pour bercer son espoir' (trad. Ernout). On voit l'opposition entre *alienae oves* 'brebis d'un autre' et *ovis pecūliaris* 'brebis laissée en possession propre à titre de *pecūlium* au berger'. Que le bien qualifié de *pecūlium* consiste ici en une brebis est simple hasard; il ne faudrait pas croire que *pecūliaris* garde un lien particulier avec *pecu* au sens de 'bétail'. *Pecūliaris* est seulement l'adjectif de *pecūlium*, et n'importe quelle possession meuble peut devenir un *pecūlium*. On le voit encore chez Plaute: un jeune garçon peut être donné comme *pecūlium* au fils du maître et sera dit le *pecūliaris puer*: c'est un des elements de la comédie des *Captivi* (v. 20, 982, 988, 1013). En fait, dans les conditions ordinaires de sa vie, l'esclave ne pouvait guère se constituer un *pecūlium* qu'avec ce qui était à sa portée; un peu d'argent, quelques moutons. Mais cette limitation de fait n'impliquait pas que *pecūlium* désignât une pièce de bétail plutôt qu'une pièce de monnaie.

Nous pouvons donc retenir *pecūlium* comme une deuxième preuve que la

notion de base, celle de *pecu*, ne se rapportait pas spécifiquement au bétail. Dans *pecūlium*, plus que dans *pecūnia*, est soulignée la relation d'appartenance personnelle, restreinte à une certaine classe sociale. Mais la possession en question est toujours celle d'un bien meuble, que *pecūlium* soit pris dans son sens strict ou dans des acceptions figurées. Par ces deux traits, possession personnelle et biens meubles, se définit aussi le verbe dérivé **pecūlo(r)* qui a produit *pecūlātus* 'appropriation (frauduleuse) des deniers publics'. Entre ce terme juridique et le terme de base *pecu*, une continuité fonctionnelle est rétablie, parallèlement à la chaîne de dérivation morphologique. Si l'on veut en effet pousser un peu plus loin cette reconstruction à la fois formelle et sémantique des liaisons entre les dérivés latins, on pourra utiliser le modèle des formes nominales de *edō*. Il a dû exister un substantif neutre en *-u *edu* (du type de **medhu *pōyu* etc . . .) prolongé par l'adjectif *edūlis* et celui-ci par l'abstrait *edūlium*. Cela fournit une chaîne parallèle à celle de *pecu*. Ainsi **edu*:*edūlis*:*edūlium* comma *pecu*:**pecūlis*:*pecūlium*. Or de *edūlium* 'plat agréable à manger' on remonte à *edūlis* 'susceptible d'être mangé' et de *edūlis* à **edu*, à peu près 'mangeaille'. De même de *pecūlium* 'possession meuble personnelle' on remontera à **pecūlis* 'appropriable', et de **pecūlis* à *pecu* qu'il faut alors définir comme 'propriété (mobilière)'. On est conduit, quelle que soit la voie choisie, à la même conclusion: *pecu* signifie 'bien meuble (personnel)'.

III. Germanique

Le mot **peku* est attesté dans l'ensemble du germanique ancien, mais le sens varie selon les dialectes et ce sont précisément ces variations qui peuvent nous instruire sur la valeur propre du terme. Nous devons l'envisager dans le contexte propre de chacun des principaux dialectes anciens.

Le vieux-haut-allemand est la seule langue germanique où **peku-*, représenté par *feho fehu fihu*, ait le sens de 'bétail'. Dans les textes traduits du latin, il rend *pecus*, *pecudes*, *iumenta*, le plus généralement *iumenta*; cf. en outre *fëhelih* 'tierisch', *fihu-stërbo* 'peste', *fihu-wart* 'Viehhirt', *fihu-wiari* 'Viehweiher'.[8]

Il est probable que les modèles latins ont agi ici comme dans bien d'autres cas. On peut constater en tout cas que vha. *fihu* s'est fort éloigné du sens que le mot avait conservé dans tout le reste du germanique. Il s'agit en effet de savoir si le vieux-haut-allemand reflète ici l'état du germanique ancien. Comme on le verra, l'innovation ou la spécialisation est à mettre au compte du vieux-haut-allemand, contrairement à ce qui est communément admis. Autrement on ne pourrait comprendre la situation qui est celle de **peku* dans les autres dialectes et qu'il reste à décrire, non plus que le rôle du terme vieux-haut-allemand lui-même dans la genèse de m.lat. *feudum* 'fief'.

Il faut en premier lieu examiner le témoignage du gotique. Le neutre *faihu* désigne seulement l'argent, la 'fortune', et n'a jamais de relation avec le monde animal. Voici les exemples au complet:

þai faiho gahabandans, *þai faihu habandans* 'ceux qui ont de l'argent, *hoi tà khrēmata ékhontes*, qui pecunias habent' (Mc 10,23; Luc 18,24);

þaim hugjandam afar faihau 'ceux qui se confient dans les richesses, *toùs pepoithótas epì khrḗmasin*, confidentes in pecuniis' (Mc. 10,24);

uas auk habands faihu manag 'car il avait de grands biens—*ên gàr ékhōn ktḗmata pollá*, erat enim habens multas possessiones' (Mc 10,22);

gahaihaitun imma faihu giban 'ils promirent de lui donner de l'argent, *epēggeílanto autôi argúrion doûnai*, promiserunt ei pecuniam se daturos' (Mc 14,11).

Ce dernier exemple suffirait à montrer que *faihu*, terme choisi pour traduire gr. *khrḗmata*, *argúrion*, lat. *pecunia*, *possessiones*, se réfère exclusivement à l'argent, à la richesse.

Enumérons à présent les composés de *faihu* au complet:

faihufriks 'avide d'argent, *pleonéktēs*, *philárguros*'

faihufrikei 'cupidité, *pleonexía*'

faihugairns 'cupide d'argent, *philárguros*'

faihugairnei 'amour du gain, *aiskhroû kérdous* (*khárin*)'.

faihugawaurki 'source de gain, *porismós*'

faihugeiro 'amour de l'argent, *pleonexía*'

faihuskul(*s*) 'qui doit de l'argent, débiteur, *khreōpheilétēs*'

faihuþraihn(*s*) 'accumulation d'argent, richesse injuste, *mamōnâs*'.

On voit donc que *faihu* est complètement étranger au vocabulaire pastoral, qui comprend des termes tout différents, tels *hairda* 'troupeau, *poímnē*, *agélē*', *hairdeis* 'berger, *poimḗn*'; *aweþi* 'troupeau, *poímnē*'; *wriþus* 'troupeau, *agélē*'; *lamb* 'mouton, brebis; *próbaton*'. L'entourage sémantique de *faihu*, ce sont les termes qui désignent l'argent et la richesse: *gabei* 'richesse, *ploûtos*', *gabeigs* (*gabigs*) 'riche, *ploúsios*' et les verbes dénominatifs *gabigjan* 'enrichir, *ploutízein*' et *gabignan* 's'enrichir', *plouteîn*' ainsi que *silubr* 'argent, *argúrion* (métal et monnaie)', *skatts* 'denier, mine, *dēnárion*, *mnâ*; plur. pièces d'argent, *argúria*'.

Une preuve supplémentaire que got. *faihu* n'a aucune attache avec le monde de l'élevage est fournie par une relation lexicale qui n'a pas encore été discernée et qu'il faut établir dans son droit.

Il existe en gotique un verbe *gafaihon bifaihon* qui traduit gr. *pleonekteîn*, avec un nom déverbatif *bifaih* '*pleonexía*'. Dans la 2ème Epître aux Corinthiens qui contient tous les exemples, Paul emploie *pleonekteîn* pour 'gagner sur quelqu'un, s'enrichir à ses dépens, l'exploiter'. C'est ce que le gotique a rendu par *bifaihon gafaihon*:

ni ainummehum gaskoþum, ni ainnohun frawardidedum, ni ainnohun bifaihodedum.

'nous n'avons lésé personne, nous n'avons ruiné personne, nous n'avons exploité personne, *oudéna epleonektḗsamen*, neminem circumvenimus' (II Cor. 7,2);

ibai . . . bifaihoda izwis? . . . *ibai ƕa bifaihoda izwis Teitus*? 'Vous ai-je exploités? Tite vous a-t-il exploités? *mḕ . . . epleonéktēsa humâs*? . . . *mḗ ti epleonéktēsen humâs Titos*?' (II Cor. 12, 17–18).

swaswe waiƕaqiss jan-ni swaswe bifaihon '(afin que votre largesse soit prête) comme une largesse, et non comme une avarice (c'est-à-dire non

comme un gain réalisé sur nous), *pleonexía*' (II Cor. 9,5); le nomin. pl. *bifaiha* en addition marginale au ms. A en II Cor. 12,20 dans une énumération de défauts: discorde, jalousie, médisance, etc. . . . (cf. Streitberg, *Got. Bibel*, p. 324 n.).

On ne peut restituer sûrement *bifaiho* ou *bifaihodai* dans le texte altéré de I Thess. 4,6 traduisant en tout cas *pleonekteîn*.

L'unique exemple de *gafaihon* est dans une glose instructive en II Cor. 2, 11: *ei ni gaaiginondau fram Satanin* 'ne nous laissons pas posséder par Satan, *hína mḕ pleonektēthômen hupò toû Satanâ*'. Le verbe *gaaiginon* 's'approprier' qui rend ici et ici seulement gr. *pleonekteîn* a suscité en glose *ni gafaihondau*, du fait même que gr. *pleonekteîn* a pour équivalent ordinaire got. *bifaihon*.

Le verbe gotique *-faihon* a été rapproché d'une famille étymologique avec laquelle il ne peut avoir aucun rapport, celle de vha. *fēhan* 'haïr', *gi-fēh* 'ennemi' *fēhida* 'querelle; guerre', all. *Fehde*, angl. *foe*. La différence considérable du sens suffirait à faire rejeter cette étymologie avec d'autres moins convaincantes encore que Feist 89 (s.v. *bifaih*) a enregistrées.

L'explication de *faihon* apparaît au sein même du gotique; *faihon* est le dénominatif de *faihu*. La formation est celle des verbes tirés de noms en *-u-*: comme *sidon*: *sidus*, ou *luston*: *lustus*. Le rapport de sens entre *faihon* et *faihu* résulte des emplois des composés de *faihu*. Puisque *faihu* désigne l'argent, la richesse, et que *faihu-friks* traduit *pleonéktēs* comme *faihu-frikei*, *faihu-geiro* rendent *pleonexía*, on a crèè un verbe denominatif *faihon* (*bi-*, *ga-*) comme équivalent de *pleonekteîn* au sens très particulier de 'gagner frauduleusement sur quelqu'un'. Ce devait être un verbe connu; c'est pourquoi quand *gaaiginondau* a été choisi une fois, contre l'usage, pour traduire *pleonekteîn*, une glose l'explique par *gafaihondau*[9] et rétablit la correspondence.

Passons maintenant au nordique.

La traduction habituelle de v. norrois *fé* par 'Vieh, Besitz, Geld'[10] doit être rectifiée: c'est la notion de 'richesse (mobilière)' qui est a mettre au premier plan. Cela ressort de trois conditions:

1. L'expression *gaggandi fé* pour 'bétail' suppose évidemment que *fé* à lui seul ne signifiait pas 'bétail', mais 'richesse, fortune'; on a désigné par *gaggandi fé*, le 'richesse sur pied', le 'bétail', tout comme gr. *próbasis*;

2. Le composé *félag* 'possession commune' d'ou *felagi* 'camarade, compagnon' (passé en v. angl. *feolaga*, angl. *fellow*) demande aussi pour *fé* le sens de 'fortune, bien', non celui de 'troupeau';

3. Le verbe dénominatif *féna* signifie 's'enrichir', donc 'acquérir de la fortune' (*fé*); d'où le dérivé secondaire *fénadr* 'richesse', désignant éventuellement le 'bétail', mais par une nouvelle spécialisation.

Relevons encore les composés: *félauss* 'sans argent'; *félitell* 'qui a peu d'argent'; *fésior* 'bourse à argent'.

Ce sont là tous les représentants authentiques de **peku* en nordique ancien. On doit en séparer radicalement deux neutres qui appartiennent à une autre famille: *fax* 'crinière' (**fahsa-*) et *faer* 'mouton' (**fahas-*) à ranger avec gr. *pekos*, *pokos* 'toison' et les formes apparentées.

En vieil-anglais, il suffit de consulter le *Concise Anglo-Saxon Dictionary* de J. R. Clark Hall et Meritt pour constater que *feoh*, au sens—traditionnellement mis en tête de l'article—de 'cattle, herd', n'a qu'un petit nombre d'attestations, qu'il y aurait d'ailleurs lieu aujourd'hui de réviser attentivement dans leur contexte, tandis que la grosse masse des examples se répartit sous les chefs: 'movable goods, property' et surtout 'money, riches, treasure'. On peut dire que *feoh* en vieil-anglais s'applique d'abord à la richesse en général ou aux biens meubles, et secondairement—parfois—à cette forme de fortune mobilière que constitue le bétail. Dans le Beowulf il signifie seulement 'richesse' ou 'trésor', et chez Aelfric la locution *wiðliegendum fēo* 'for ready money' confirms l'ancienneté du sens.

Les nombreux composés de *feoh* ont la meme distribution statistique.

Avec *feoh* au sens de 'cattle': *feoh-bigenga* 'cattle-keeper'; *feoh-gōd* 'property (in cattle)'; *feoh-land* 'pasture'.

Mais avec *feoh* 'argent, richesse': *feoh-behat* 'promise of money'; *feoh-bōt* 'money compensation'; *feoh-fang* 'taking a bribe'; *feoh-gafol* 'usury'; *feoh-gehāt* 'promise of money'; *feoh-georn* 'covetous, greedy'; *feoh-gerefa* 'steward'; *feoh-gesceot* 'payment of money'; *feoh-gestealdd* 'possession of riches'; *feoh-gestreon* 'treasure, riches'; *feoh-gīfre* 'avaricious'; *feoh-gif* 'largess'; *feoh-gītsere* 'miser'; *feoh-gītsung* 'avarice'; *feoh-gyrnes* 'greed'; *feoh-hūs* 'treasure, house'; *feoh-lǣnung* 'lending of money'; *feoh-leas* 'without money'; 'not to be bought off'; *feoh-lēasnes* 'want of money'; *feoh-sceatt* 'money-payment'; *feoh-spēda* 'riches'; *feoh-spilling* 'waste of money'; *feoh-strang* 'well off'; *feoh-wite* 'fine for coining false money'; *fēolaga* 'partner, fellow' (cf. v. nor. *félagi*); *feohhord* 'treasury'; *feohlufu* 'love of money'.

Au total trois composeés avec *feoh* 'betail', une trentaine avec *feoh* 'argent, richesse'.

On peut répéter l'observation pour le moyen-anglais en étudiant les articles *fe* du Middle English Dictionary de Kurath-Kuhn (III 430). Il y a très peu d'exemples du sens mis en tête: 'live stock', mais beaucoup plus de *fe* comme 'movable property; possessions in live stock, goods or money, riches, treasure, wealth' et comme 'money as a medium of exchange or as used for taxes, tribute, ransom, bribes, etc.'

Il faudrait procéder à un nouvel examen des examples en se libérant du schéma traditionnel qui imposait à tout prix 'bétail' comme sens initial, et classer les emplois dans leur exacte valeur contextuelle. Cette révision aurait probablement quelques conséquences pour l'histoire de l'anglais *fee* et celle de fr. *fief*, ancien *feu*. L'étymologie et l'évolution du mot français *fief*, si important pour la théorie de la féodalité, ont été excellemment présentées par K. J. Hollyman.[11] Mais, sous l'empire de la tradition, cet auteur suppose que francique *fehu* 'bétail' est devenu lat. *feus* 'biens mobiliers'. Il cite lui-même (*op. cit.* p. 43) la glose 62 de Cassel: *fihu*:*pecunia*. Signifie-t-elle que le bétail servait d'argent? On penserait plutôt que *fihu*, comme got. *faihu*, désignait toute forme de biens meubles et a gardé ce sens quand il est passé en latin. Ici aussi un nouvel examen serait nécessaire.

IV. Conclusions

Cette esquisse a montré que la conception traditionnelle du **peku* indo-européen doit être entièrement réformée.

Notre première conclusion est que **peku* signifie 'possession mobilière personnelle'. Que cette possession soit, en fait, représentée par le bétail est un fait distinct, qui tient à la structure sociale et aux formes de la production. C'est seulement par suite de cette association fréquente entre le terme **peku* et la réalité matérielle de l'élevage que, en se généralisant hors de la classe des producteurs, **peku* en est arrivé à signifier 'bétail'—première spécialisation—, puis spécifiquement 'petit bétail'—deuxième spécialisation—, et enfin 'ovin', —troisième et dernière spécialisation. Mais en lui-même **peku* ne dèsigne ni le troupeau ni aucune espèce animale.

Nous pouvons établir alors une corrélation entre le sens propre de **peku*, ainsi restauré, et sa distribution dialectale. Il est intéressant de noter—les comparatistes n'y ont pas prêté assez d'attention—que **peku* manque en grec. Ce n'est pas un hasard. Une notion aussi importante ne pouvait simplement disparaître. Le terme indo-européen a été, en fait, remplacé en grec par une désignation nouvelle, qui porte le même sens; c'est hom. *próbasis*, avec son équivalent beaucoup plus commun, *próbata*.[12] Le sens des termes grecs se déduit de leur formation: *próbasis* est littéralement la 'possession marchante', c'est-à-dire les biens meubles, valeur qui s'oppose à celle des *keimêlia*, des 'biens immeubles'. Or *próbata* désigne en fait, comme on sait, le 'bétail'; d'abord n'importe quel bétail, puis spécialement le 'petit bétail', enfin le 'mouton' (cf. gr. moderne *próvato* 'mouton'). Nous avons ici, explicite, le modèle de l'évolution que nous posons pour **peku*: au point de départ, un terme désignant la 'possession mobilière', qui, pour des raisons extra-linguistiques, se trouve appliqué fréquemment à la possession de bétail; il devient alors le terme pour 'bétail' et ultérieurement pour l'espèce de bétail prédominante, le 'mouton'.

Mais l'intérêt du problème est que cette spécialisation, accomplie tôt sur le domaine indo-iranien, ne s'est pas réalisée partout. Nous avons en latin et au moins dans une partie notable du germanique, soit dans l'emploi du mot, soit dans ses dérivés, des témoins de haute antiquité, prouvant que le sens premier était conservé ou qu'il avait suivi une autre évolution. Il est en effet improbable au dernier degré que **peku*, s'il avait signifié proprement 'bétail', eût pu désigner 'l'argent' et la 'fortune' en général, comme font lat. *pecūnia* et got. *faihu*.

Ce sera notre deuxième conclusion: dans un procès lexical de cette nature, c'est un terme de sens général qui se trouve appliqué à une réalité spécifique et qui en devient la désignation, et non l'inverse. Nous prenons ici le contre-pied du rapport qui a été posé depuis les étymologistes latins jusqu'à nos récents dictionnaires, entre *pecu* et *pecūnia*.

Il est en effet de règle que les termes qui se rapportent à des formes diverses de possessions sont des termes généraux, dénotant leur relation au possesseur, mais n'indiquant rien sur la nature propre de la chose possédée. La signification générale permet ainsi des désignations spécifiques, qui, au long

de l'histoire, finissent par s'attacher si étroitement a leur objet particulier que le sens originel en est oblitéré. Nous en avons un exemple clair dans gr. *próbasis, próbata.* En voici quelques autres, empruntés à des langues et à des cultures variées.

Le terme spécifique anglais *cattle*, fr. *cheptel* remonte a lat. *capitāle* 'bien principal'. Déjà dans un texte de 1114 apparaît *captale* 'chattel, cattle, movable goods'.[13] Mais encore au Moyen-Age *capitale* a le sens de 'fortune, biens, revenus', et l'espagnol *caudal* signifie 'biens, richesses'. Le passage 'fortune mobilière > bétail' est caractéristique. Mais une fois accompli, il est irréversible.

Gr. *ktênos* et surtout pl. *ktênea*, proprement 'acquêts', et 'richesse' en général, devient en ionien le terme pour 'richesse en bétail', puis pour 'bétail'; par suite, le singulier *ktênos* pourra désigner une tête de bétail (gr. mod. *ktínos* 'bête').

Le point de départ est le même dans l'espagnol *ganado*, litt. 'possession gagnée', devenu la désignation du 'bétail'; ainsi que dans irl. *indile* 'propriété' et 'bétail', et gall. *da* 'bien, propriété' (substantivation de *da* 'bon'), devenu 'bétail'; *gwartheg* 'valeur, richesses' et 'gros bétail'; anglais *live stock* 'bétail', a. fr. *vif aveir* 'bétail'.

Latin *res* 'chose, possession' a donné en espagnol *res* 'tête de bétail'; et dans une grande partie de l'Amérique du Sud, esp. *hacienda* est devenu la désignation du 'bétail' (communication de A. Tovar). En polonais *bydło* 'bétail' est un dérivé de **bhū-* et suppose un sens plus ancien 'état, possession', cf. tchèque *bydlo* 'habitation' (communication de J. Kuryłowicz).

Dans plusieurs des langues influencées par l'arabe, en persan, en turc, en mongol, le mot arabe *māl* 'possession' a pris entre autres le sens de 'bétail'.

En hébreu, *miqneh*, dont le sens étymologique est 'acquisition, possession' (de la racine *qnh* 'acquérir, posséder') désigne ordinairement le 'bétail'; il en est de même de *qinyān* 'possession', qui est le nom du 'troupeau'.[14]

On voit de toutes parts les confirmations de ce principe que le terme général seul est celui qui fournit les désignations particulières, mais que les désignations particulières, une fois établies, ne peuvent plus s'élargir. Ainsi le 'bétail' est très souvent désigné par les termes qui se rapportent à la possession en général, c'est-à-dire qu'on le désigne simplement comme 'possession'. A partir de slave *blago* 'bien', le serbocroate crée *blâgo* 'bien, fortune, bétail'. Nous ne voyons pas d'example du procès inverse.

Notre interpretation de **peku* et de son évolution est donc conforme à ce qu'on pourrait appeler la norme des termes de possession: une appellation générale ou générique est employée dans une certaine classe de producteurs comme désignation de l'objet ou élément typique; en cette qualité il se diffuse hors de son milieu originel et devient alors la dénomination usuelle de l'objet ou élément en question. On a pu, par l'examen comparé des données dans trois groupes dialectaux, suivre pour **peku* les étapes de ce procès, et vérifier dans une certaine mesure cette reconstruction interne.

Une dernière conclusion touche à l'étymologie de **peku-*. Si la présente démonstration est jugée recevable, elle ruine le rapprochement traditionnel avec **pek(t)-* "tondre". Il est évident que **peku*, terme de valeur économique ne dénommant aucun animal, ne peut rien avoir de commun avec des termes dérivés de **pek(t)-* qui sont propres à la technique de la tonte et du peignage de la laine: gr. *pékō* 'peigner, carder', *pókos* 'toison', *pektéō* 'tondre', *pékos* n. 'toison', *pokízō* 'tondre la laine', *kteís* 'peigne'; lat. *pectō* 'peigner, carder' *pecten* 'peigne', *pexus* 'velu, cotonneux'; arm. *asr* 'laine'. Entre ces formes et **peku* il n'y a rien d'autre qu'une ressemblance.[15] Le rapprochement doit être abandonné, et **peku-*, vestige du plus ancien vocabulaire indo-européen, semble irréductible à aucune racine connue.

Notes

[1] Nous réservons les formes baltiques, qu'on suspecte d'être empruntées au germanique ou à quelque autre langue de l'ouest.

[2] Le rapprochement peut être fait avec des textes comme *RV.* X 85.42 (Poultney, *The Bronze Tables of Iguvium* 255), mais alors il est beaucoup moins significatif, s'appliquant à tous les vivants indistinctement.

[3] Lüders, *SBAW.* 1917, 368; dans le même sens pour l'italique, Sittig *KZ.*52 (1924) 210; pour l'avestique, Gershevitch, *The Avestan Hymn to Mithra*, 1959 182.

[4] M. Bloomfield, On some disguised forms of Sanskrit *paçu* 'cattle', *IF.* 25, p. 185.

[5] Fastes, V, 277 sq.

[6] *De lingua latina* V 92. Mais il n'y a rien a retenir des speculations étymologiques de V 97 (*pecus* derive de *pascō* et de *pes*, etc. . . .).

[7] Ainsi J. B. Hofmann *Lat. etym. Wb. s.v. peculium* donne correctement 'zu eigen' comme sens premier, d'apres un article de Pokrowski, *Symbolae Rozwadowski* 1 (1927) 223, tout en appréciant inexactement le rapport avec *pecu*.

[8] Toutes les citations et références utiles sont réunies par H. Palander, *Die althochdeutschen Tiernamen* (1899) 13–15.

[9] Pokorny, *Idg. etym. Wb. 797 s.v. pek-*, cite W. Wissmann, *Die ältesten Postverbalia* 79 pour une explication de *faihon* par *faihu*. N'ayant pas vu ce livre, je ne sais si les raisons qu'il donne s'accordent avec les miennes.

[10] Ainsi, J. de Vries, *Altnord. etym. Wb.* 114.

[11] *Le développement du vocabulaire féodel en France pendant le Haut-Moyen-Age*, (Paris, 1957) 41 sq.

[12] Sur ce mot et sur les problèmes des désignations analogues, nous renovoyons à *BSL* 45 (1949) 91 sq., en reprenant ici quelques données utiles pour notre démonstration.

[13] Baxter-Johnson, *Mediaeval latin word-list* 64.

[14] Cf. Koehler-Baumgartner, *Lexicon*, 561 a et 844 b.

[15] Je regrette d'avoir autrefois reproduit ce rapprochement par concession à l'opinion commune (*Origines* 50–1).

Studies in Indo-European Legal Language, Institutions, and Mythology

Calvert Watkins

> Sprachforschung, der ich anhänge und von der ich ausgehe, hat mich nie in der weise befriedigen können, dass ich nicht immer gern von den wörtern zu den sachen gelangt wäre; ich wollte nicht blos häuser bauen, sondern auch darin wohnen.—Jakob Grimm

We are fortunate in possessing an enormous body of legal texts in most of the earlier Indo-European languages, ranging from India to Ireland, from Scandinavia to the Mediterranean and Anatolia. These legal texts sometimes are among the earliest extended documents attested in these languages, as in Rome or Ireland; but nearly everywhere they share the common stylistic feature of reflecting earlier, more archaic linguistic usage than contemporary texts from other areas of discourse than the law. That in some cases we are dealing with what is technically termed pseudo-archaism (as in Cicero's *De legibus*) is not relevant here; the archaism is in either case real enough.[1]

The conservatism of legal language in the various Indo-European linguistic traditions (modern as well as ancient) has been long recognized, and scarcely needs comment. Given the singular importance of verbal formulae, the fiction of the immutability of the law, and ultimately the necessary continuity of a legal tradition, this innate conservatism of legal linguistic usage is readily explained, and indeed fully justified.

Yet the implications of this conservatism for the historical linguist have hardly begun to be realized or appreciated. The comparison and contrast between legal and nonlegal linguistic usage in a given language affords a superb field for the application of the techniques of internal reconstruction, which is the foundation of linguistic history.

There is a further aspect of the problem. The linguist studying legal language must here as elsewhere be concerned with content as well as form; with 'legal meaning', and by extension, with a whole cultural subsystem. It is to be expected that an understanding of this special cultural context may frequently provide the key to the proper linguistic interpretation of a given form; we shall have an instance below in the first item to be discussed, Latin *ūsurpāre*.

There is still another case to be illustrated by the second of our examples, Latin *noxiam sarcīre*, which is more serious in its implications. The linguist

may well succeed in reconstructing a legal technical term—the form together with the particular cultural context which gives it meaning—from the evidence of more than one cognate language. At this point he must realize that he has reconstructed for the common language not only an etymon, but a part of the structure of nonmaterial culture of the speakers of that language. Given linguistic evidence in the Indo-European languages for common etyma in form and function for a given social or legal institution—and an obvious example is the system of kinship terms—it is a necessary fact that social or legal institution existed in the society of speakers of Common Indo-European. To deny this is to deny to the reconstructed language the status of a vehicle of human communication.

This implication may then be recast in its proper historical context: the linguist who makes such a reconstruction, by the techniques of the comparative method, from the data of legal language in the various Indo-European languages, has thereby furnished incontrovertible formal evidence for the common Indo-European origin not merely of a given vocable, but of salient features of the different historical legal systems themselves. The linguist at this point finds himself in a wholly new domain: that of comparative law in the genetic sense.

That there should be a close analogy between the comparative method in language and in the law follows from the identity of the problem in each. Any linguistic comparatist will state that there are basically three possible explanations for similarity between languages: borrowing or diffusion, common inheritance, and independent creation due either to universality or to chance. Within the field of jurisprudence, let us cite the words of the great romanist and comparatist Leopold Wenger:[2] 'Und doch muss sich jede Rechtsvergleichung, wenn sie Gleichheit oder Ähnlichkeit verschiedener Rechte festzustellen in der Lage ist, über die Gründe solcher Erscheinung Rechenschaft zu geben versuchen. Drei Hauptmöglichkeiten werden da zu erwägen sein: *Rezeption*, *gemeinsame Herkunft*, *unabhängig gleichartige Rechtsbildung*.'

Comparative law in the genetic sense has fallen rather into the background, after a considerable florescence in the nineteenth and early twentieth centuries; genetic comparison and the search for Indo-European antiquities gave way to a more broadly based, but purely typological comparison, which is the common acceptation today of the term 'comparative law'. Yet the linguist can make a significant contribution to the genetic comparison of legal institutions, beyond that of providing the sort of evidence just discussed.

Any historical approach to customary law, indeed any attempt to write the history of institutions, must as in the case of the history of languages operate with the basic concept of an ordered structure undergoing change. It is in the domain of language change that the only proper application of the comparative method has been developed, that which is grounded on structuralism in its highest sense: the search for recurrent similarities not in isolated features, but in the relations which integrate a set of features.

It is this approach to the history of institutions which the linguist can contribute now. The use of the comparative method was virtually coeval in Indo-European linguistics and the study of Indo-European antiquities; witness the work of Jakob Grimm, or later Adalbert Kuhn. But the refinements and renewal of the method in linguistics did not extend to the study of antiquities, and it is this which explains in large part the frequently unsatisfactory, at times ludicrous[3] results of the latter. It remains to be seen whether the more modern techniques can prove effective in such a domain as comparative law; the example of comparative mythology would suggest so. Thus the third study in our series, on a distinctive feature approach to the law of theft, is frankly speculative. It is offered primarily to stimulate discussion, not as a finished piece, and the occasion of this conference seemed an appropriate vehicle.

The fourth and final study is only tangentially connected with legal language or institutions. Rather it attempts to suggest an equation in the domain of Indo-European comparative mythology, on the basis of a principle of social symbolism; specifically that a Hellenic and a Vedic deity, Hermes and Pūṣan, together represent a transposition into mythology and religion of the notion of exchange and reciprocity. We will have several occasions below to refer to the classic work of Marcel Mauss; our final study is yet another testimony, however humble, to the fecundity of his contribution to anthropology and sociology.

I. usurpare

Latin *ūsurpāre* and derivatives, principally *ūsurpātiō*, have in the literary language the primary meaning of 'take into use, make use of, use' (Lewis and Short, *s.v.*). In this sense the verb is attested as early as Plautus; cf. Cist. 505 *inter novam rem verbum usurpabo vetus* 'in the midst of a new affair I will use an old proverb.' By the time of the empire, *ūsurpāre* comes to be almost a synonym of *ūtor*, over which it had the advantage of being active, not deponent, and taking an accusative, not an ablative object.

Ernout and Meillet, *Dict. étym. de la langue latine*[4] s. v. *ūtor*, recognized that this use of *ūsurpāre* in literary Latin must be secondary, and represent a semantic transfer or generalization from an originally specific and technical acceptation. They suggest that the original meaning was in all likelihood connected with the legal institution of marriage: 'Terme de droit, qui peut-être s'est employé d'abord de celui qui prenait une femme (rapere) sans passer par des noces légitimes; cf. Gell. 3.2.12 sqq.' This view was accepted by Hofmann, and appears almost verbatim in his edition of Walde's *Lateinisches etym. Wörterbuch*. The formal analysis which this presupposes is set forth by Vendryes in a study of *ā*-verbs in Latin, *MSL* 16.300 (1910): a compound **ūsu-rapāre* (with regular syncope of the *a*), where the second member is to *rapere* as *oc-cupāre* (*ob-cap-*) is to *capere*. From the formal point of view, such an analysis is irreproachable in Latin morphology, whether we assume original *ūsu* + *rapāre* or (more likely) a denominative in *-āre* from a compound noun *ūsu-rap-*. Indeed as early as 1885, Bréal and

Bailly in their *Dict. étym. lat.*, s.v. *ūtor*, derive *ūsurpāre* from a noun **ūsu-rapos*.

Yet it is from the semantic side that we may have serious doubts about this derivation. For the relation between the two members of the compound is by no means clear. On the analogy of *ius dicere* → *iudex*, *signa ferre* → *signifer*, etc., the productive type of compound with verbal rection in Latin, we should expect *ūsurp-* to be a transformation of **ūsum rapere*. Yet it is hard to imagine what the meaning of such an expression would be; it makes no sense in Latin. For this reason, historians of Roman law have suggested an underlying **usui rapere* 'seize for one's use'.[4] But in Latin where the underlying sentence is other than verb + object, a nominalization does not yield a true compound, but a collocation, as in *ūsūcapiō* (*ūsū capere*).

If the interpretation 'prendre une femme sans passer par des noces légitimes' were in fact justified, surely we should expect the woman to be the object of the verb, rather than *usus*. Such an expression does exist in Latin; *virginem rapere* is the technical term for the ancient and legally recognized Indo-European institution of marriage by abduction, *Raubehe*.[5] Cf. Livy 1.9.10 (describing the rape of the Sabines) *signoque dato iuventus Romana ad rapiendas virgines discurrit*. We have a literary reminiscence in Catullus' use of the phrase in the wedding song 61.3–4: *qui rapis teneram ad virum/virginem*.

Elsewhere in Indo-European, we have the identical 'demoniacal' marriage-rite (*rākṣasa vivāha*) of Hindu law Manu III 24:

> *hatvā chittvā ca bhittvā ca krośantīm rudatīm gṛhāt prasahya kanyāharaṇan rākṣaso vidhir ucyate*
> 'The forcible abduction of a maiden from her home, while she cries out and weeps, after (her kinsmen) have been slain or wounded and (their houses) broken open, is called the *rākṣasa* rite' (tr. Bühler).

Note the technical term for making 'outcry' *krośati*, as to a thief caught in flagrante delicto; for further discussion see section three below.

In Irish law we have the 'union of abduction', *lánamus foxail*, listed in Cáin Lánamna §§ 4, 34, the tract on marriage-law.[6] Though this text does not give a detailed description, we have ample documentation of the practice in the Old Irish sagas.

Finally in Hittite law we have what may be called the 'wolf-marriage'. As set forth in the Hittite Code, no action lies for murder arising out of an attempted abduction, and the situation is covered by the doubtless shouted formula *zik-wa* URBARRA-*aš kištat* 'you have become a wolf'. I § 37:[7]

> *takku* SAL-*nan kuiški pittinuzzi n*[*u* . . . . *šard*]*ieš appan anda*[8] *pānzi*, *takku* 3 LÚMEŠ *našma* 2 LÚMEŠ *akkanzi*, *šarnikzil* NU.GAL *zik-wa* URBARRA-*aš kištat*
> 'If someone abducts a woman a[nd . . . . help]ers go after them, if 3 or 2 men die, there is no compensation; "you have become a wolf".'

Yet nothing either in literary, historical, or legal Latin texts suggests that the institution of marriage by abduction has anything to do with the action denoted by *ūsurpāre*. Nor is a derivation from *rapere* in the least supported by the text of Aulus Gellius to which Ernout and Meillet refer, as will appear below.

We have, however, full information on the precise legal meaning of *ūsurpāre* in connection with marriage. The texts are explicit, and one may be permitted to wonder why no mention of this information appears in the linguistic literature. Let us examine the first member of the compound *ūsurpāre*: in a compound legal technical term we expect at least one of the components to be likewise a technical term, and the answer is afforded by an archaic sense of the word *ūsus* in legal Latin. It designates the simplest of the three oldest forms of legal marriage in Rome, by which the wife came *in manum viri*, under the power of her husband: continuous cohabitation for a year. The locus classicus is Gaius, *Inst.* I §§ 109–111:[9]

> 109. Sed in potestate quidem et masculi et feminae esse solent; in manum autem feminae tantum conveniunt. 110. Olim itaque tribus modis in manum conveniebant: usu, farreo, coemptione.
> 111. Usu in manum conveniebat quae anno continuo nupta perseverabat; quia enim veluti annua possessione usucapiebatur, in familiam viri transiebat filiaeque locum optinebat. Itaque lege duodecim tabularum cautum est ut, si qua nollet eo modo in manum mariti convenire, ea quotannis trinoctio abesset atque eo modo [usum][10] cuiusque anni interrumperet. sed hoc totum ius partim legibus sublatum est, partim ipsa desuetudine oblitteratum est.

> '109. Now, while both males and females are found in *potestas*, only females can come under *manus*.
> 110. Of old, women passed into *manus* in three ways, by *usus*, *confarreatio*, and *coemptio*.
> 111. A woman used to pass into *manus* by *usus* if she cohabited with her husband for a year without interruption, being as it were acquired by a usucapion of one year and so passing into her husband's family and ranking as a daughter. Hence it was provided by the Twelve Tables that any woman wishing not to come under her husband's *manus* in this way should stay away from him for three nights in each year and thus interrupt the usus of each year. But the whole of this institution has been in part abolished by statutes and in part obliterated by simple disuse' (tr. de Zulueta).

It is precisely to this institution, the *usurpatio trinoctii*, that the passage of Aulus Gellius (3.2.12) cited by Ernout and Meillet refers. It is worthwhile examining the whole text:

> quoque Mucium iureconsultum dicere solitum legi non esse usurpatam mulierem, quae, cum Kalendis Ianuariis apud virum matrimonii causa esse coepisset, ante diem IV. Kalendas Ianuarias sequentes usurpatum

isset: non enim posse impleri trinoctium, quod abesse a viro usurpandi causa ex duodecim tabulis deberet, quoniam tertiae noctis posterioris sex horae alterius anni essent, qui inciperet ex Kalendis.

'I have read that Quintus Mucius, the jurist, also used to say that a woman did not become her own mistress who, after entering upon marriage relations with a man on the Kalends of January, left him, for the purpose of emancipating herself, on the fourth day before the Kalends of the following January; for the period of three nights, during which the *Twelve Tables* provided that a woman must be separated from her husband for the purpose of gaining her independence, could not be completed, since the last six hours of the third night belonged to the next year, which began on the first of January' (tr. J. C. Rolfe).

One should note in this passage the consistent intransitivity of the verb *ūsurpāre*, which is more ancient than the use of the word in Plautus.

It is important to comprehend the legal notion of *manus*, the position of the wife in the ancient Roman family, and the nature of *ūsus*; for therein lies the solution to the etymology of *ūsurpāre*. We can do no better than to cite the words of the great Sir Henry Maine, to whom that etymology was evident in 1875:[11]

'From very early times it would appear to have been possible to contract a legal marriage by merely establishing the existence of conjugal society. But the effect on the wife of continuous conjugal society was, in Old Roman law, precisely the same as the effect on a man of continuous servile occupation in a Roman household. The institution called Usucapion, or (in modern times) Prescription, the acquisition of ownership by continuous possession, lay at the root of the ancient Roman law, whether of persons or of things; and, in the first case, the woman became the daughter of the chief of the house; in the last case the man became his slave. The legal result was only not the same in the two cases because the shades of power had now been discriminated, and paternal authority had become different from the lordship of the master over the slave. In order, however, that acquisition by Usucapion might be consummated, the possession must be continuous; there was no Usucapion when the possession had been interrupted—where, to use the technical phrase (which has had a rather distinguished history), there had been *usurpation*, the BREAKING[12] of *usus* or enjoyment. It was possible, therefore, for the wife, by absenting herself for a definite period from her husband's domicile, to protect herself from his acquisition of paternal power over her person and property. The exact duration of the absence necessary to defeat the Usucapion—three days and three nights—is provided for in the ancient Roman Code, the Twelve Tables.'

This implicit recognition of the etymology of *ūsurpāre* is in fact far older than Maine; it was recognized by the Roman jurists themselves. Compare the passage from Gaius cited above, and especially Paulus in the Digest

41.3.2, who shows a remarkable understanding of the true history of the word: *usurpatio est usucapionis interruptio; oratores autem usurpationem frequentem usum vocant* 'usurpation is the interruption of usucapion; but the orators call frequent use usurpation.' We have here not only a definition, but an etymological gloss, of a type well known in Roman law, and common to the Irish legal commentators as well. Most of these etymologies are admittedly fanciful, though linguistically interesting as metalanguage; but a few are fundamentally correct, such as Papinian's *adulterium . . . propter partum ex altero conceptum* (Digest 48.5.6.1). So it is in the case of usu*rpatio* < usu*capionis inter*rup*tio*. It should be evident that the correct analysis is *ūsurpāre* < **ūsu-rup-āre*. The second member of the underlying nominal compound *ūsu-rup-* is the hitherto unattested root noun to Lat. *rumpere*: literally, 'breaking *ūsus*'.

Usurpatio thus in its oldest attested meaning in Latin, prior to the time of the Laws of the XII Tables, is the breaking of *usus*: the suspension of the orderly acquisition of ownership, by the interruption of continuous possession. We have all the elements of a structural system in ancient prehistoric Roman law, with a remarkably exact, homologous linguistic expression. Continuous possession as a subjective possibility, 'faculté de se servir',[13] is *ūsus*; the objective, effective acquisition of ownership thereby is *ūsūcapiō* < *ūsū capere* 'take by *ūsus*'; but the effective suspension of the acquisition of ownership thereby is *ūsurpātiō* < *ūsu-rup-* 'breaking *ūsus*'.

I would suggest that it is the passage from intransitivity to transitivity which primes the classical meaning of *ūsurpāre* as 'use'. At the outset we had an intransitive verb *ūsurpāre* in the meaning 'break *ūsus*' and specifically 'interrupt the legal period of one continuous year necessary for acquiring *manus* over a woman', as attested in Gellius. The word *ūsus* in this compound I assume to be the technical term for the simplest form of marriage. From this was formed the noun *ūsurpātiō* 'interruption of *ūsus*', and an adjectival derivative *ūsurpātus* 'whose *ūsus* has been interrupted'—quite possibly femininum tantum. But an originally only adjectival phrase *mulier usurpata est* is formally ambiguous, adjective + copula or perfect passive: cf. Kuryłowicz, *Inflectional categories* 56ff. It can at any time be derived from a **mulierem usurpare* by reversing the apparent passive transformation, whence *aliquid usurpare*.

Alternatively, one can also imagine that from an original action or agent noun **ūsur(u)p-s*, or perhaps a feminine **ūsur(u)p-a* 'she who breaks *ūsus*, was formed a purely nominal but quasi-passive derivative *ūsurpāta* 'she whose *ūsus* has been broken' (somewhat parallel to the semantic relations of English *divorcée*), and that the verb *ūsurpāre* is itself a back-formation from the adjective in *-ātus*.

That the verb becomes a virtual synonym of *ūtor* is probably to be accounted for as a simple 'misuse', conditioned by phonetic and vague semantic resemblance (cf. Eng. *hypothecate* improperly for *hypothesize*[14]), outside of the specific legal context to which it was originally restricted. For the meaning 'seize for one's own use' it is possible that there existed a folk

etymological connection with *rapere*, as Cowgill suggests to me (*per litt.* 21 VI 1966), though I have not been able to find any textual support, and that meaning of *ūsurpāre* is in any case relatively late (Imperial) in Latin. More plausible would seem to me to explain it by the factor of sound symbolism; a final labial stop or nasal is extremely characteristic of words meaning 'take, seize, hold' or the like in a variety of languages. Cf. Lat. *rap-iō*, *cap-iō*, *carp-ō*, Gk. harpázo, Skt. *grabh-*, Russ. *xap-at'*, Hitt. *ep-zi*, OIr. *gaib-id*, or Eng. *grab*, *cop*, *clasp*, *nab*, and for the principle, Jakobson, *IJSLP* 1-2.274(1959). It is worth noting that this principle is evident in Indo-European itself, *teste* such root forms as **ep-*, *rep-*, **kap-*, **ghabh-*, **grebh-*; and **klep-* 'steal (in secret, v. *infra*)' is doubtless formed on the **kel-* of Lat. *cēlāre*, *occulere* plus a suffix *-ep-* of the same symbolic origin.

II. aut noxiam sarcire aut noxae dedere

One of the most characteristic features of Roman law, present during all of the preclassical and classical period, was the institution known generally to Romanists as the *actio noxalis*, or noxal action. For its classical form, we may cite Gaius, *Inst.* IV 75:

> Ex maleficio filiorum familias servorumque, veluti si furtum fecerint aut iniuriam commiserint, noxales actiones proditae sunt, uti liceret patri dominove aut litis aestimationem sufferre aut noxae dedere. 'Wrongdoing by sons or slaves, as where they have been guilty of theft or outrage, has given rise to noxal actions, the nature of which is that the father or master is allowed either to bear the damages awarded or to surrender the offender' (tr. de Zulueta).

As de Zulueta points out in his commentary *ad loc.* (pp. 271–3), the original technical term for the restitution of the damage done was *noxiam sarcire*; Gaius' *litis aestimationem sufferre* is the result of an innovation in procedure. We have the ancient formula *aut noxiam sarcire aut noxae dedere* (whence the title of this paper) in a slightly modernized form in Ulpian, *Digest* 9.1.1: *aut noxam*[15] *sarcire aut in noxam dedere.*

The legal system underlying the noxal action, and its historical development, has been discussed and analyzed at great length by the Belgian jurist F. de Visscher, in his monumental work *Le régime romain de la noxalité* (1947). The original situation is one where theft or damage committed by an animal or by an individual *in potestate*, in other words someone with no legal capacity of his own (slave, son, doubtless originally wife) requires the offender to be handed over by the master to the victim (noxal surrender, *noxae deditio*). The master can, however, avoid this by offering restitution for the damage done (*noxiam sarcire*). Speaking of the law of the XII Tables, de Visscher states: 'à la victime il reconnait le droit d'exiger la *deditio*, au maître une faculté de rachat, dont l'efficacité est assurée par l'effet libératoire de l'offre de paiement.'

In the historical development of noxality, de Visscher recognizes three stages (p. 20). The first, which recurs in numerous societies around the world, consists of noxal surrender of the offender pure and simple. The

second, qualified quite rightly as a 'legal system', combines the institution of noxal surrender with the master's right of compensatory restitution. The third, that of classical Roman law, presents the developed system of regular legal actions (*actiones noxales*) superimposed on the earlier second system as an optional alternative. This last need not concern us here.

De Visscher argues that the first stage, that of noxal surrender alone, is still attested in Roman tradition in international or intertribal politics; he gives a detailed juridical analysis of the affair of the Furculae Caudinae and the *deditio* of the consuls Veturius and Postumius (Livy IX 1–12). But the solemn religious character of the international *deditio* and its specifically expiatory function is evident from the role of the *fetiales*; cf. the archaic formula in Livy IX 10.9. This would lead us to suspect that the international *deditio* was something more than an equivalent in macrocosm of the relations between families. The same is suggested by the spontaneous nature of the *deditio*, especially in the affair of the Furculae Caudinae: the Samnites neither demanded nor accepted the *deditio*; nor had they been offended or injured by the consuls. The Fetial formula has *noxam nocuerunt*, but the *noxa* itself was not to the Samnites. When we further note that between families, noxal surrender is always of an animal or an individual who is not *sui iuris*—for which there is no real equivalent on the tribal level—one is strongly inclined to conclude that de Visscher's first and second stages of *deditio* or noxal surrender in fact represent different and coexistent institutions, rather than a historical development of the same institution. As will appear, the comparative evidence likewise favors such a view.

It is clear that the second stage, the "legal system" requiring noxal surrender of the offending animal or individual *in potestate* to the victim of the offense, but which may be averted or 'bought off' by the restitution of the damages, was already in full force before the time of the XII Tables. The texts are explicit. They are collected in Bruns, *Fontes*[8], and have been commented in detail by de Visscher; it is therefore not necessary to cite more than a single characteristic passage.

We have in XII 2a *si servus furtum faxit noxiamve no*[*x*]*it*, (preserved in the *Digest*), and it is extremely plausible that the isolated *sarcito* quoted by Festus (p. 330 L) belonged in the same article; for the syntax compare *si im occisit, iure caesus esto*. Festus states that *sarcito in XII Ser. Sulpicius sit significare 'damnum solvito, praestato'*. Whether we are to understand *damnum solvito* as a gloss on *noxiam sarcito*, or on a presumably intransitive *sarcito* alone, is uncertain; but we can observe in the language of the jurists that *noxia* is equivalent to and gradually replaced by *damnum*.

In any case we have clearly attested in Latin an antithetical pair *noxiam nocēre/noxiam sarcīre*. The way to its analysis is shown by the classical *Essai sur le don* of M. Mauss, the implications of which for Indo-European studies have been so clearly drawn by Benveniste in his article 'Don et échange dans le vocabulaire indo-européen'.[16] We may do no better than to quote Benveniste's introduction:

C'est le grand mérite de Marcel Mauss, dans son mémoire désormais

classique sur le Don, d'avoir mis en lumière la relation fonctionnelle entre le don et l'échange et défini par là un ensemble de phénomènes religieux, économiques et juridiques propres aux sociétés archaiques. Il a montré que le don n'est qu'un élément d'un système de prestations réciproques à la fois libres et contraignantes, la liberté du don obligeant le donataire à un contre-don, ce qui engendre un va-et-vient continu de dons offerts et de dons compensatoires. Là est le principe d'un *échange* qui, généralisé non seulement entre les individus, mais entre les groupes et les classes, provoque une circulation de richesses à travers la société entière. Le jeu en est déterminé par des règles, qui se fixent en institutions de tous ordres. Un vaste réseau de rites, de fêtes, de contrats, de rivalités organise les modalités de ces transactions.

In a society founded on the notion of exchange and reciprocity, an offense (*noxa*), either to gods or men, requires a counterprestation. It is *sarcire* which denotes precisely the counterprestation which reestablishes the balance, which completes the circle of 'donner, recevoir, rendre'. As Mauss has stated about the Indians of the Pacific Northwest (p. 203 n.3): 'L'expiation consiste précisément à donner un potlatch ou au moins un don. C'est là, dans toutes ces sociétés, un principe de droit et de rituel extrèmement important. Une distribution de richesses joue le rôle d'une amende, d'une propitiation vis-à-vis des esprits et d'un rétablissement de la communion avec les hommes.' The fundamental meaning of *sarcīre* is its legal and social meaning, that of making restitution or reparation: 'making whole', but specifically thereby restoring the social unity and equilibrium disturbed by an offense. It is this sense alone which is anchored in the sociological fact of exchange and reciprocity as the fundamental structural principle. All the other acceptations of *sarcīre* in Latin, whether of repairing ('making whole') shoes, walls, or whatnot, represent a secularized, desocialized, and entirely secondary transfer. This conclusion is offered by what is surely the oldest use of the verb in Latin: the archaic formulas *sane sarcteque* and *sarcta tecta*. We may note the alliteration and asyndeton, as well as the earlier phonetic form of classical *sartus*. Both are found in Festus and Paulus, pp. 428–9 Lindsay: ⟨*sa*⟩*rte in augu*⟨*ralibus pro inte*⟩*gro ponitur*: ⟨"*sane sarctequ*⟩*e audire vi-*⟨. . . *ob quam causa*⟩*m opera pu*⟨*blica, quae locantur, ut i*⟩*ntegra praes*⟨*tentur, sarcta tecta v*⟩*ocantur. et*⟨*enim sarcire est integr*⟩*a facere.* See also Charisius (Keil 1.220), ultimately from the same source but who gives the fuller version of the formula: *hinc 'sarcta tecta uti sint' opera publica* [*publice*] *locantur, et ut Porphyrio ex Verrio et Festo 'in auguralibus' inquit 'libris ita est, sane sarcteque. . . '.* In the solemn dedication of a new building there is of course no question of 'repairs': the meaning is 'may they be whole'.

De Visscher has shown in the work already cited that in legal usage *noxa* = *delictum*, and specifically 'l'état durable de responsabilité résultant de la commission du délit' (p. 544).[17] On the other hand, *noxia* = *damnum*, 'le dommage sujet à réparation' (p. 562 n. III); it is properly an adjectival derivative in *-i̯o-*, 'that of the *noxa*'. The confrontation with *noxia* permits

us also to be more precise about the value of *damnum*, in the direction indicated by Benveniste (note 16 above). It is basically parallel with the root **alg^{w}h-* of *alphánō*, which as Benveniste has shown, is not simple 'get', but 'get in exchange'; *alphḗ* is the 'valeur d'échange'.[18] Latin *damnum* is not simply 'damage' in the physical sense of destruction of property, etc.; it is specifically 'damage entailing liability (for restitution or reparation)', and as such clearly retains the notion of reciprocity, of exchange implicit in the root **dap-*.

The etymological connection of Lat. *sarcīre* with Hitt. *šarnikzi* (with nasal infix) 'makes restitution' goes back to A. Juret, *RHA* 2.251 (1934). The equation has been universally accepted by Hittite scholars, Latinists, and Indo-Europeanists alike;[19] alone among Latin etymologists, Ernout and Meillet make no mention of the Hittite verb in their discussion of *sarcio*, probably because of their erroneous view that 'le sens primitif était sans doute "recoudre" '.

I do not propose to question this etymology, but rather to demonstrate it once and for all from the conclusive evidence of legal texts in the two languages. For Hitt. *šarnikzi* is, like Lat. *sarcīre*, the expression for 'making restitution' PRECISELY IN THE LEGAL SYSTEM OF NOXALITY.

The institution of noxality is clearly documented for Hittite society, in a fashion wholly parallel to Rome, in the Hittite Code. The article in question (I § 95) is found in the Old Hittite version A, and thus goes as far back in Hittite legal tradition as we have documentation; cf. note 5 above. For a detailed discussion one may be referred to the important study 'Zur Noxalhaftung im hethitischen Recht' by H. Petschow, *ZA* N.F.21.237–50 [1963] and the references therein. (The author does not note the critical parallel with Latin.) The texts are as follows, in the readings of A where available:

I § 95

takku ÌR-*aš* É-*ir taizzi šakuwassar-pat pāi tayazilaš*[20] 6 GÍN KUBABBAR *pāi* ÌR-*ša* KAxKAK-*ŠU ištamanuš-šuš kukkuriškizzi n-an appa išḫi-šši pianzi takku mekki taizzi mekki-še išḫiyanzi takku tepu taizzi tepu-šše išḫianzi takku BEL-ŠU tezzi šer-šit-wa*[21] *šarnikmi nu šarnikzi takku mimmai-ma nu* ÌR-*an-pat šuizzi*

'If a slave robs a house he gives back in full. He gives 6 shekels of silver as compensation for theft, and he [the victim] cuts off the slave's nose and ears, and they give him back to his master. If he steals much, they bind much on him; if he steals little, they bind little on him.[22] If his master says "I shall make restitution for him", he makes restitution; but if he refuses, then he surrenders the slave.'

I § 99

takku ÌR-*aš* É-*ir lukkizzi išhaš-šiš-a šer-šit šarnikzi . . . takku natta-ma šarnikzi nu apūn-pat šuizzi*

'If a slave sets a house on fire and his master makes restitution for him . . . but if he does not make restitution, then he surrenders him'.

The parallel with the Roman system is thus complete, and the two verbs occupy identical positions in the structure:

nocēre	*sarcīre* / *dēdere*	=	*taizzi*	*šarnikzi* / *šuizzi*

Hittite *šarnikzi* is likewise used for requiting an offense to the sphere of the divine, as appears from the Plague Prayers of Mursilis:[23] I rs. 19–20: *ammuk-ma šumaš ANA* DINGIR.MEŠ EN.MEŠ-*YA šarnikzel parkann-a* KUR-*e šer šarninkiškimi* 'Ich aber werde euch, den Göttern, meinen Herren, Ersatz und Sühne wegen des Landes leisten'. Comparison of this and the preceding passage from the Code above shows that the verb should be given in the lexicon as compounded with a preverb, *šēr šarnik-*.[24] It may occur intransitively, as in the passages above, or may take an object, like *noxiam sarcire*: Pestgebet I Rs. 21–2 DINGIR-MEŠ *BELU*.MEŠ-*YA* ŠA ^{I}*Dutḫaliya kuit ešḫar* EGIR-*an sanḫatteni nu-kán* ^{I}*Dutḫaliyan kueš kuennir nu ešhar apūs šarninkir*. 'Ihr Götter, meine Herren, weil ihr das Blut des Dutḫaliyaš rächen wollt: die den Dutḫaliyaš töteten, die haben die Blutschuld gebüsst.'

We can thus make a functional and formal equation of Lat. *sarcīre* and Hitt. *šarnikzi*; it is significant that the institution of noxality is in fact the only point of their semantic overlap, but that here the two recover each other completely. The formal details of the equation need not detain us: Hitt. *šarnikzi* is a regularly formed nasal infix present from a root **serk-* (or state II **srek-*), **sr̥-n-ek-ti*. Lat. *sarcīre* is evidently a derived present in *-yo/ī-* to a base *sark-*; the sigmatic perfectum and verbal adjective to *-to-* are regular and predictable. The base *sark-* probably continues a root noun **sr̥k-*, with morphological zero grade in *a*, according to the mechanism described in Kuryłowicz's *Apophonie* 174ff. To judge from **bhreg-*, **bhr̥-n-(e)g-* > **bhrang-* > *frangō*, a **serk-* (*srek-*) with nasal infix **sr̥-n-(e)k-* (= Hitt. *šarnik-*) should give **srank-* > a Lat. **francō*. We may conjecture that the phonetic distance of such a form from the denominative *sark-yo-* (replacing **sr̥k-yo-*) was at a certain time responsible for its elimination.

Before passing to the implications of this etymology in its cultural context, let us look at the expressions for the noxal surrender proper: *noxae dedere*, ÌR-*an-pat šuizzi*.

In the Latin verb *dēdere* we have a good example of the preverb *dē-* marking a transfer of status, as Benveniste has defined it (in lectures, 1958): the offender ceases to be under the jurisdiction of the paterfamilias, and comes under the jurisdiction of another. Hittite *šuizzi* is also to be so interpreted, in the meaning 'surrenders, transfers permanently, without recourse'. In this sense we have the solution to the much vexed problem of the common phrase in the Hittite laws *parna-šše-ya šuizzi* (*šuwāizzi*), which must mean 'and he surrenders (it, the 'fine' or compensation) to his (the plaintiff's) household'.[25] The Hittite verb is very likely related to OIr. *soid* 'turns' (both from **soṷi̯eti*), though the latter does not to my knowledge have any comparable legal significance.

Let us return to the equation of Lat. *sarcīre* and Hitt. *šarnikzi*. G. Dumézil has seen clearly that the role of linguistic equations in the domain of comparative mythology is relatively restricted. This is exact; the question is primarily one of comparing structures and the interrelations of their constituents. But when we can independently compare social structures and their linguistic expression, we have furnished precious and conclusive evidence for the common—in our case Indo-European—origin of both. In the domain of comparative mythology, examples are the safely restorable **deiu̯o-* and **di̯eu- pəter-* (**pəter- di̯eu-*).

Human institutions may show similarities attributable to nongenetic factors: chance, (quasi-)universality, or diffusion (borrowing). It is this that the comparative method must be continually on guard against; and the danger is incontrovertibly greater in the comparison of institutions than in the comparison of languages. For these reasons it is useful to have some sort of "touchstone" by which to judge the impressionistic comparisons or equations that may be made between institutions, legal or otherwise. One of these touchstones is precisely that of linguistic equations of the kind made here between Lat. *sarcīre* and Hitt. *šarnikzi*: IE **serk-* (*srek-*)/*sr̥k-*. But it is important to note that what is decisive is not simply the linguistic equation—which is after all only a root etymology, and scarcely very impressive as "straight" linguistic equations go—; it is the COINCIDENCE of *linguistic expression* and *legal content*. Specifically, we may term it the coincidence of the linguistic expression of a particular legal structure point, a "nexus" in two legal structures which may be compared and equated, point for point and relation for relation, in two cultures where mutual influence is impossible both for chronological and geographical reasons. We have here such an example; noxal surrender, and even the system of noxality as a whole, is not confined to Indo-European speaking societies. But it is the existence of a common inherited lexical item *sarcīre/šarnikzi* occupying the same position in the same legal structures which proves that the institution of noxality in Indo-European society must be as old as the community of Latin and Hittite, which is to say at the present state of our knowledge that it belongs to the common Indo-European period.[26]

It should be unnecessary, but is perhaps still worthwhile, to point out explicitly that a necessary consequence of the preceding discussion is that Hittite law is at least in part inherited from Indo-European customary law. This means that the culture area notion of 'cuneiform law'[27] can no longer be considered as a wholly adequate frame of reference within which to discuss Hittite law, synchronically or diachronically.

In view of the foregoing evidence from Latin and Hittite it is scarcely surprising that there is independent evidence from other early Indo-European traditions for the system of noxality. Thus from Greek we may cite Plato's Laws 936d: *ho toû blápsantos despótēs ḕ tḕn blábēn exiásthō . . . ḕ tòn blápsant'autòn paradótō*;

'Let the master of the offending slave either make restitution for the damage . . . or hand over the offender himself.'[28] From a genuine legal docu-

ment we may cite the following, from the *Code of Gortyna* II.20–36 (Schwyzer 179, Buck 117): *ai ka . . . moikiōn ailethēi . . . proweipatō . . . tō de dōlō tōi pastai anti maiturōn duōn. ai de ka mē allusētai, epi tois elonsi ēmen krēththai opai ka leiōnti.* 'If he is caught in adultery . . . , one shall announce (it) . . . in the case of a slave to the master before two witnesses. But if he is not ransomed, it shall be in the power of the captors to do with him as they wish.' It is well known that Roman tradition saw a partial 'reception', or any rate a significant influence, from Greece of the Law of the Twelve Tables, and scholars have deduced a number of quite convincing phrasal reminiscences.[29] But it is the equation of Latin *sarcīre* with Hittite *šarnikzi* (where "classical" influence is out of the question), which proves that the Roman system of noxality must be independent from that of Greece.

In early Slavic law the possibility of diffusion from Byzantium must of course be reckoned with. But there is enough of Slavic, particularly Russian law in direct contrast with Roman usage (e.g. the law of murder) that its evidence should at least be heard. M. Kaser *Röm. Privatrecht* 2.311, notes that the institution of noxality was on the decline both in the Western and the Eastern Roman empire; its comparative significance in the early Slavic codes would strongly suggest an independent tradition. In the oldest manuscript (thirteenth century) of the *Russkaja Pravda*, Expanded Version (ed. E. F. Karskij, Leningrad, 1930) article 121 reads: *aže xolopъ kradetъ kogo ljubo to g(ospodi)nъ vykupati i ljubo vydati i* 'if a slave robs a man, the owner may redeem or surrender him' (tr. Vernadsky). Likewise in the tenth century Bulgarian *Zakon Sudnyj ljudem*, our earliest Slavic legal text, but one where Byzantine influence is most suspect, we have the same proviso. In the normalized text of the Short Version, ed. M. N. Tixomirov (Moscow [1961]), §27: *kraduščemu rabu gospodinъ, ašče xoščetъ iměti takovago raba, da bes toščety stvoritъ; ašče li ne xoščetъ iměti togo raba sego da otdastъ v rabotu okradenomu* 'The master of a thieving slave, if he wishes to have such a slave, let him make [compensation] without loss; if he does not wish to have the slave, let him hand him over into slavery to the one who was robbed.'

III. The Roman Law of Theft: A Comparative Linguistic Approach

Like other aspects of culture in general, such an area of the law as that of theft is not merely the sum total of the list of relevant statutes or juristic opinions of one sort or another, though it may frequently be manifested in that way alone; it is a structural unity, an internally consistent and coherent whole. As such, *ex hypothesi*, its internal organization may be analyzed in terms of a set of binary oppositions, of 'distinctive features' in the technical linguistic sense of the term. For structural method and the principle of binary oppositions as applied in anthropology one may refer to Cl. Lévi-Strauss, *Anthropologie structurale* (1953). The same author has a pioneering analysis of a Pacific Northwest Indian myth into such binary oppositions in La geste d'Asdiwal, *Annuaire de l'École prat. des hautes études, Sect. des sci. relig.*, 1958, and has now expanded and refined the analysis in his brilliant work *Mythologiques. Le cru et le cuit* (1964). In the same tradi-

tion is the remarkable new monograph of Vjač. Vs. Ivanov and V. N. Toporov, *Slavjanskie jazykovye modelirujuščie semiotičeskie sistemy* (1965). It is this notion of structure which I suggest can be applied with profit to the field of law.

The earliest recorded Roman law of theft is set forth in a number of quotations, paraphrases, and reminiscences of the laws of the Twelve Tables, collected for the most part under the conventional rubric of Table VIII. They are most conveniently accessible in Bruns, *Fontes*[8]; a fuller, indeed nearly exhaustive philological and juridical analysis of these may be found in chapter I of the excellent work of P. Huvelin, *Études sur le furtum dans le très ancien droit romain*, 15–103 (1915). It is not my intention—nor within my power—here to attempt a complete analysis of the system of the earliest Roman law of theft, nor of its connections with other aspects of the law. Rather, my object is to attempt a preliminary sketch of the structural features underlying certain aspects of the earliest Roman law of theft, specifically the *furtum manifestum*, and to show that these recur almost exactly in the early legal systems of numerous other Indo-European speaking peoples. The similarities are such that, in accord with the basic tenets of the comparative method, this set of features of the law of theft may be attributed to the common legal system that obtained in the society of speakers of Indo-European itself.

That I have taken Roman law (albeit Archaic, not Classical) as the point of reference, is largely attributable to the simple fact that it represents the most thoroughly investigated of all the legal traditions of the Indo-European speaking world, and is the most familiar to students of comparative law. Such a justification is scarcely proper from a scientific point of view; in the last analysis I can only appeal to tradition, and recall the words of Leopold Wenger (*op. cit.* 9): 'Mag die darin liegende Minderbewertung . . . aller anderen Rechte als des römischen überwunden sein, eines ist bei aller Vergleichung noch geblieben: wenn man nach einem vergleichenden Masstab . . . sucht um zu vergleichen, so ist das römische Recht auch heute . . . das weitaus reichhaltigste, . . . der beste juristische Wertmesser und das beste Verständigungsmittel bei aller Rechtsvergleichung der verschiedensten Rechte untereinander.'

I have purposefully limited myself to Roman, Greek, Indic, and Slavic legal systems as comparanda; the rich Germanic, and even more archaic Celtic traditions have not been cited, in order not to swell beyond all measure an already lengthy study. But the same distinctive feature mapping will hold for both the Germanic and the Celtic systems, as D. A. Binchy informs me.

It is specifically theft which interests us here; but it must be noted at the outset that theft proper is only one member of the opposition theft (*furtum*)/robbery (*rapina*). The distinctive feature is the clandestineness and secrecy of the action: *nocte latent fures* (Catullus 42.34); cf. the relation *furtum*:*furtim*, or for that matter English *steal*:*stealth*. This opposition is very likely of Indo-European antiquity; it recurs in Greek *klopḗ* vs. *harpagḗ*, and in Germanic between the family of Goth. *hlifan* and that of *bi-raubon*.

In the early Roman law of theft proper the fundamental opposition is that between *furtum manifestum* and *furtum nec manifestum*. The binary, 'yes/no' character of this distinctive feature is evident from the linguistic expression, as is the fact that *furtum manifestum* is the marked member of the opposition. Yet the archaic form of the negation *nec* testifies to the great antiquity of this curiously modern structural interpretation. The same form is found elsewhere in Roman law, as in the opposition of *res mancipi* and *res nec mancipi*.[30] *Furtum manifestum*, as the marked member of the opposition, was the more serious, and was more severely punished; *furtum nec manifestum* was sanctioned only by restitution and compensation. So in the Twelve Tables (VIII 16) *si adorat furto, quod nec manifestum erit—duplione damnum decidito*. The latter clause is conjectural, after Tab. XII 3; but the double compensation itself is clear from the introduction of Cato's *De agri cultura: maiores nostri . . . in legibus posiuerunt furem dupli condemnari*. See the discussion in Huvelin, *op. cit.* 77, who also argues very plausibly from Lucilius 552–3 Marx, 582–3 Terzaghi, that the procedure of an action *furti nec manifesti* began with an *in ius vocatio*.[31]

Furtum manifestum itself was either simple or complex, depending on whether the thief was discovered in the act, or by tracking (Lat. doubtless *investigatio*), and the solemn search for the stolen goods in his house, *quaestio lance licioque* (*cum lance et licio*). The two were legally equivalent. As we shall see below, one of the distinctive features of manifest theft is *enclosure*: catching the thief within the yard or house, not in the open field. This house-search may be viewed as effecting a juridical equivalence of the thief discovered within *your* enclosure, and your stolen goods discovered within *his* enclosure: two alternative manifestations of the same distinctive feature. We would have in this system an excellent illustration of what Lévi-Strauss has termed 'le caractère relationnel de la pensée symbolique'.

This solemn, juridically recognized house-search was regulated as a ritual. The searcher had to be naked or nearly so; he carried with him a platter (*lanx*) and a tether (*licium*), which together symbolize the stolen articles: artifacts and foodstuffs, which could be (at least symbolically) carried on the platter, and chattels, which could be tethered, were in early Indo-European society the principal objects subject to theft.[32]

Catching a thief *in flagrante delicto* doubtless belongs to the plane of legal universals. But the solemn, ritual house-search does not; yet it is attested as a legal institution also among the Greeks, the Germanic peoples, and the Slavs (Jolowicz, p. 171, with references). Cf. the Scandinavian technical term for this house-search (ON *ransak*), which was borrowed into English. For this reason it has been quite rightly assumed by legal historians that this search was 'a very ancient institution, dating back to a time before the separation of the Indo-Germanic peoples, for so many parallels are found that direct borrowing is most unlikely' (Jolowicz, *loc. cit.*). [Cf. also C. V. Schwerin, *Die Formen der Haussuchung in idg. Rechten* (1924).]. This institution is peculiarly Indo-European, and for this reason valuable evidence;

but from the structural point of view it is the legal equivalence as *fur manifestus* among the Indo-European speaking peoples of the thief discovered in this peculiar way and the thief caught in the act, which permits us to recover one feature of the *system* of the Indo-European law of theft. The *quaestio lance licioque*, however precious its probative value for its very peculiarity, is only one element in the total structure.

We come finally to the specific legal provisions in which we are directly interested: the case of *furtum manifestum* where the thief is caught in the act. It will be observed that this situation up to this point may be represented by the model of a 'tree' or branching diagram; we have, in effect, passed through three successive nodes, representing the choices *furtum* (not *rapina*), then *f. manifestum* (not *f. nec. m.*) then *f. m.* caught in the act (not by the house-search *lance licioque*). As will appear below, the same model will handle the remainder of the law of theft manifest.

The first two relevant articles in the Twelve Tables are as follows, after Bruns: (VIII 12) *si nox furtum faxsit, si im occisit, iure caesus esto*, (VIII 13) *luci . . . si se telo defendit, . . . endoque plorato.* The first is evidently a textual citation; it is found in Macrobius, *Sat.* 1.4.19 introduced by *verba haec sunt.* The second is more conjectural, and more of a patchwork; it seeks only to give those words from various sources which may be reasonably presumed to have stood in the original article. In fact, only *luci* and *endoque plorato* may with any confidence be regarded as authentic. The general disposition of the law is clear from the paraphrases in Cicero (*P. Tull.* 20.47): *atque ille legem mihi de XII Tab. recitavit, quae permittit, ut furem noctu liceat occidere, et luci*, si se telo defendat. (*Ibid.* 21.50): *furem . . . luce occidi vetant XII tab . . . nisi se telo defendit, inquit; etiamsi cum telo venerit, nisi utetur telo eo ac repugnabit, non occides; quod si repugnat, endoplorato, hoc est conclamato, ut aliqui audiant et conveniant.* (*P. Mil.* 3.9): *XII tab. nocturnum furem quoque modo, diurnum autem si se telo defenderet, interfici impune voluerunt*

The form *luci*, found in the first passage in Cicero, recurs in a line from Ennius, *Ann.* 431 Vahlen, beginning *si luci, si nox*, where the rare and archaic adverbial *nox* 'by night' after *si* makes it likely that we have here in Ennius a reminiscence of the Twelve Tables. Ennius, like Cicero, would have learned the Twelve Tables *a parvo* as a *carmen necessarium.*

The very archaism of Cicero's *endoplorato* makes it reasonable to presume its authenticity; but the even more archaic syntagma is cited in Festus p. 402: *'sub vos placo' . . . ut in legibus 'transque dato' et 'endoque plorato'.* The unique circumstances of this juridical cry make it almost certain that *endoque plorato* comes from the law of theft, and thus that Cicero simply left out the *-que* for which there was no syntactic need.

To these two passages a third from the Twelve Tables must be added, which furnishes a further disposition in the law of theft. It is arbitrarily assigned to Table XII, but surely belongs with the two articles from Table VIII. (XII 2a) *si servus furtum faxit noxiamve no[x]it.* The context is that of the system of noxality, which has been amply discussed in the preceding

section. We have here an important feature of all early legal systems: *status*, and the fundamental opposition free/not-free. We should perhaps say rather slave/not-slave; for it is the not-free who is the marked member of the opposition, as is again clear from the linguistic expression. We need only juxtapose *si servus furtum faxit* and *si (nox) furtum faxit*; in the latter the free man, as the unmarked category, is simply not specified.

Though nowhere explicitly stated, it seems likely that the feature of status, *servus/liber*, is relevant only in the case of *furtum nec manifestum*, where there would be some sort of trial. In the case of *furtum manifestum*, both free man and slave could under certain circumstances be slain with impunity; here the opposition was thus neutralized.

Across these fragmentary legal texts and literary paraphrases we can clearly discern all the relevant features of an organized structural system. However much we may regret the incompleteness of our information about the Twelve Tables in general, it would appear that as far as theft is concerned, despite our uncertainty in matters of detail and particularly in the question of procedure, we have the essence of the law preserved. Taken individually, the legal texts as we have them appear to be only simple conditional sentences, preserved for us in a purely haphazard way. But considered together, they constitute a system with a definite purpose: they supply a set of distinctive features, the specification of which (as yes or no choices) in turn fully and uniquely specifies the juridical nature of the given instance of theft, and as such, the course of action to be followed in its sanction.

In the particular case which interests us, that sanction is the legal right to kill the thief with impunity, as an exercise of private justice. As F. Wieacker states in his important study, Endoplorare: Diebstahlsverfolgung und Gerüft im altrömischen Recht, *Münch. Beitr. z. Papyrusforsch. u. ant. Rechtsgesch.* (*Festschrift L. Wenger*) 34.129–79 (1944): 'Die Diebstötung ist nicht Rache- oder gar Notwehrhandlung, sondern vielmehr rechtmässige Unrechtsabwehr' (p. 158). At the time of the Twelve Tables, 'impunity' would mean that the slayer would not by his act become a *paricidas*, a murderer. At an earlier period this impunity would imply the nonoccurrence of a blood-feud; the kin group of the slain thief would not have the right (or rather obligation) of revenge upon the slayer.

The provisions of the law are therefore clear. In Jolowicz's words (172) 'If the thief came by night or if in the daytime he defended himself with a weapon the XII Tables allowed him to be killed without trial; in the latter case however the slayer must cry aloud in order to prevent any suspicion that he was a murderer attempting to hide his act.' We have here a path through successive choices, representing the specification of the relevant features.

The first feature is not noted by Jolowicz; but its structural relevance is clear from the discussion by Wieacker, op. cit. 164. The thief can be slain with impunity only if caught in the act within an enclosure, e.g. house, barn, or barnyard. Wieacker suggests very plausibly that when Cicero

(*P. Tullio*, v. supra) specifies *intra parietes tuas*, he is giving a juridically relevant condition. We know that theft in an open field, even by night, was sanctioned in an entirely different way, having nothing to do with the exercise of private justice. We have the testimony of Pliny, *Nat. Hist.* 13.3.12 (= Tab. VIII 9): *frugem quidem aratro quaesitam furtim noctu pavisse ac secuisse puberi XII tabulis capital erat, suspensumque Cereri necari iubebant.* In the crucifixion of the thief as an expiation to Ceres we should probably see an ancient religious law, which might well—pace Pliny—have had no connection at all with the laws of the Twelve Tables.

The next choice is the opposition *by night/by day*; *nox/luci*. Given a plus specification of the features 'enclosure' and 'night', the juridical course of action is completely specified: the thief may be slain with impunity, *iure caesus*. But in the case of a minus specification of the feature 'night', i.e. *luci*, it is necessary to specify two further features, in other words, to pass through two more successive nodes: the features of 'armed resistance' by the thief, *si se telo defendit*, and 'outcry' by the victim, *endoque plorato*. Only in the event of a plus specification of both these remaining features can the *fur diurnus* be slain with impunity, *iure caesus*.

Cicero (*loc. cit.*) states explicitly that the mere possession of a weapon is not adequate: *inquit etiam, si cum telo venerit, nisi utitur telo ac repugnabit, non occidi*. 'Armed resistance' or 'violence', as we saw earlier, is a redundant feature of *rapina*; but in the case of the *fur diurnus* it is fully relevant and distinctive.

There is some question as to whether the outcry was a necessary concomitant of the slaying of the *fur nocturnus* as well; see Wieacker, *loc. cit.*, and Jolowicz, p. 558, with the literature there cited. But from the structural point of view this is only a question of the ordering of the distinctive features: whether 'outcry' is specified before or after 'night/day'. Though some uncertainty persists for the period of the Twelve Tables, comparative law would suggest that the outcry was originally a necessary concomitant of all action taken in cases of *furtum manifestum*, even including the house-search *lance licioque*. In terms of our model, this implies that the feature 'outcry' was specified some nodes before the feature 'night/day'.

Clamor, the juridical outcry, is well attested at an early period throughout the Indo-European speaking world. The institution as reflected in early texts has been studied at great length both from the juridical and the philological point of view by Wilhelm Schulze, *SbPAW* (1918) 481–511 (= *Kleine Schriften* 160–89), and by L. L. Hammerich, *KDVS*, hist. -fil. medd. 29.1 (1941). The reader may be referred to these works for numerous illustrations. Schulze concludes that the juridical outcry (Germ. *Gerüft*) is an Indo-European heritage, while Hammerich, weighing much the same evidence, regards *Gerüft* simply as a 'juristische Verwendung des Allarmrufes' created only in the Germanic High Middle Age. The evidence is all for Schulze's view. In the first place, we have the linguistic equation of the technical term for the 'juridical outcry' in Sanskrit *krośa* and Old English *hréam* < Gmc. **hraugma-* (later replaced in English by the now 'secu-

larized' *hue and cry*). But of greater importance is the position of the outcry in the legal structure. The significance—and probative value—of the outcry as an institution of Indo-European date lies precisely in the fact of its being articulated and integrated into a total legal system, of which it is only one component. It is the close similarity of the structural integrations of the outcry in cognate legal systems in the Indo-European speaking world which represents the primary comparative datum, the equation which permits us to make a reconstruction of the state of affairs in Indo-European. The more primitive evolutionary bias which seeks to derive the juridical outcry from a 'simple' cry for help or cry of alarm actually—and artificially—removes the outcry from its status as an integrated social fact, and as such essentially deprives it of any interest or value whatsoever. The situation is exactly the same in another area of anthropology, where Lévi-Strauss has eloquently shown that we must take marriage by exchange as the primary social fact, 'seulement un aspect d'une structure globale de réciprocité qui fait l'object d'une appréhension immédiate et intuitive de la part de l'homme social', rather than seeking to derive it from promiscuity, consanguineous marriage, or group marriage, as did Frazer.[33]

The resultant system, then, for this aspect of the Roman law of theft at the time of the Twelve Tables includes the following features, listed by the key Latin word or phrase for the marked member of the opposition:

1. furtum
2. manifestum
3. lance licioque
4. servus
5. endoque plorato
6. intra parietes
7. nox
8. telo

The given set of yes or no (+/−) specifications of each of these features is thus a complete description of the juridical nature of any given act of theft. It represents all the legally relevant information and as such determines the appropriate sanction; it is at once descriptive and prescriptive. Jolowicz has stated (p. 177) that 'the XII Tables, like other primitive systems, are detailed and rigid; the code tries to lay down exactly what is to happen in every case, so that when the question of guilt is decided nothing is left to the judge.' In principle this is quite correct; but a structural analysis shows that the detail and rigidity, the *specificity* of the law is on a much deeper level than, for example, the fixed monetary penalties Jolowicz cites as an instance. The latter, in fact, testify to a relatively late and far advanced development.

It is possible finally to show that many of these distinctive features recur elsewhere in the system of Roman law: just as in phonology, these oppositions are the elemental structural units, the 'building blocks' of the system.

Thus *furtum manifestum/f. nec manifestum* may be put more generally as an opposition *present/absent* (*not* present): (*furtum*) *manifestum* is only a

contextual variant. Another variant of 'present' occurs in the law of procedure in the Twelve Tables. Proper 'court' procedure requires that both parties to the action be present, *ambo* praesentes (Tab. I 1, 9): the opposition must in a certain sense be neutralized. Where one of the two is not present, the case is adjudged by default to the party present, the marked member of the opposition: (I 8) *post meridiem praesenti litem addicito.*

The opposition *night/day* is only one feature of a larger system of divisions of time, different elements of which are relevant ('distinctive') in different areas of the law. The specification 'by night' recurs, as we have seen earlier, in Pliny's *noctu pavisse ac secuisse* (Tab. VIII, 9). In the law of procedure (I 7–9), we have another temporal opposition: *ante meridiem/ post meridiem.* These represent the divisions of day; the limits of day—beyond which a case cannot go—are expressly defined in the law of procedure (I 9): *sol occasus suprema tempestas esto.*

The feature of status, as in all early legal systems, is of paramount importance. Both members of the opposition *servus/liber* are explicit in Tab. VIII 2, from the law of *iniuria*: *si os fregit libero, CCC, si servo CL poenam subito* (in all likelihood not the original wording). The free man is explicit in the law of Numa Pompilius (Bruns 12): *si qui hominem liberum dolo sciens morti duit, paricidas esto.* From the comparison of the latter two cases with the law of theft an interesting principle can be inferred: where the slave is the offender, as in the case of theft, he is the marked member of the opposition; but where the slave is the victim, the injured party, he is the unmarked member of the opposition, and as in the law of Numa need not be explicitly specified.

The general category of status was of course further elaborated, as we would in any case expect in any early Indo-European society. Other distinctions of status were legally relevant in different areas of the law; it is scarcely necessary to mention the legal aspects of the opposition between the sexes, or the position of an individual *in potestate* and the whole question of legal capacity. For another case, compare the opposition *adsiduus/ proletarius* (both free) in the law of procedure (I 4): *absiduo vindex adsiduus esto*; *proletario iam civis qui volet vindex esto.* I give the text as amended by F. Wieacker, *loc. cit.*, who quite rightly reads *civis qui* for *civi quis*, since it would be the civic status of the *vindex*, not of the *proletarius* which would be juridically relevant. The structural simplicity of this legal formulation is remarkable: its author clearly saw that only the *vindex* of the *absiduus* need be specified; for the other, the unmarked number of the opposition, he lets *civis qui volet* 'anyone, so long as he is a citizen' be *vindex.* It is perhaps not too farfetched to see in this keen structural 'sense' of the early jurists the true foundations of Rome's greatest and most lasting contribution to world culture: her jurisprudence.

The feature of *outcry* is of particular interest, and indeed deserves a separate study. There are in early Roman law and in early Roman society in general an enormous number of formal, legal public outcries, each with its own particular sphere of use, and usually with its own lexical expression.

From the Twelve Tables, we may note (I 6) *rem ubi pacunt, orato*; (II 3) *cui testimonium defuerit, is tertiis diebus ob portum obvagulatum ito.* A *kalator* is mentioned in our oldest epigraphic fragment of Roman sacred law (Bruns, p. 14), though the mutilated context precludes our knowing what his function was. Outside the laws proper, we have the imaginative *flagitatio* in Catullus 42, where E. Fraenkel has so clearly shown the relation to Italic popular justice (*Kl. Beiträge* 2.121ff = *JRS* 51.49ff. [1961]). We may point out a large number of special technical uses of *nuntiare* and its compounds, each of which denotes a specific sort of legal or ritual public outcry or proclamation. Cf. *bellum denuntiare, alicui testimonium denuntiare, aves nuntiare, renuntiare* 'announce the election of', and others, which have never been studied as a group and certainly merit such an investigation. All of these public outcries in Roman culture are contextual variants of a single feature, each conditioned by its own sphere of application. In the law of theft, he who kills a thief without making an outcry (*endo plorare*) is a murderer (*paricidas*); in the law of nations, we may compare Cicero, *Rep.* 2.17: '. . . *ut omne bellum quod* denuntiatum *indictumque* non esset *id* iniustum esse *atque impium iudicaretur.*' The structural relation of the public outcry, *endo plorare* or *denuntiare* is the same in either case.

We are now in a position to look more closely at the comparative evidence: the law of theft in the early legal systems of the various Indo-European speaking peoples. It will not be possible to go as fully into these systems as we have into that of Rome; but it is proposed that the relatively brief textual citations from legal documents will be sufficient to show that to a remarkable extent the same structural features recur in the same configurations as in early Roman law. When the occasional innovations are abstracted from these various legal doctrines, we are left with a coherent system which may be regarded as common patrimony.

For Indic law, the far greater part of our information, and the totality of our 'legal' texts proper, the *smṛti*, belongs to a relatively late period, posterior to the Vedic epoch. The law as it is known to us in Indian culture has already undergone a long and properly Hindu development. Yet despite the numerous religious innovations, as a result of which Hindu law superficially resembles *ius canonicum* rather more than *ius civile*, it still clearly preserves sufficient features reflecting an earlier state of affairs to be extremely valuable as comparative legal evidence. This is particularly evident in civil and criminal law.

By the time of the 'Laws of Manu', the Mānavadharmaśāstra, the exercise of private justice has been almost completely restricted or eliminated, and the function of punishing those guilty of delicts has already devolved upon the public authority, the king. The right to slay with impunity exists only in cases of self-defense (VII 349–51). Yet clear traces of the earlier system remain, in the death penalty for the thief caught *in flagrante delicto*, and explicitly for the nocturnal housebreaker:

na hoḍhena vinā cauram ghātayed dharmiko nṛpaḥ/sahoḍhena sopakaraṇaṃ ghātayed avicārayan

(IX 270). 'A just king shall not cause a thief to be put to death, (unless taken) with the stolen goods (in his possession); him who (is taken) with the stolen goods and the implements (of burglary), he may, without hesitation, cause to be slain' (tr. Bühler).

A subsequent passage gives the further specification:

saṃdhiṃ chittvā tu ye cauryam rātrau kurvanti taskarāḥ/teṣāṃ chittvā nṛpo hastau tīkṣṇe śūle niveśayet.

(IX. 276) 'But the king shall cut off the hands of those robbers who, breaking into houses, commit thefts at night, and cause them to be impaled on a pointed stake'.

Here *rātrau* 'by night' exactly recovers Lat. *nox*; we have the same distinctive feature, with the specification of the marked member, as in Rome. The punishment of cutting off the hands is doubtless homoeopathic; one may note that the same verb is used for house-*breaking*, (*saṃdhiṃ*) *chittvā*, as for *cutting off* the hands of the malefactors, *chittvā* (*hastau*). The two verbs likewise occupy the same position in the line. We need only replace the king (*nṛpa*), the symbol of public justice, by the injured party in the historically antecedent system of private justice, to recover the Indo-European system intact. In the form of the Sanskrit directive to slay, we have an exact semantic equivalent of the Latin *iure caesus esto*: *ghātayed avicārayan*, rendered by Bühler 'he may, without hesitation, cause to be slain', could be equally well if not better translated 'he shall slay without departing from the right'.

In Hindu law as well we find the distinctive feature of enclosure. It is implicit in the technical legal expression *saṃdhiṃ chittvā* of the preceding passage from Manu, the equivalent of our 'breaking and entering'. And in the Gautamadharmaśāstra, perhaps the oldest of our Hindu legal texts, and the only one entirely in prose, we have the following passage (XII.28): *go 'gnyarthe tṛṇam edhān vīrud vanaspatīnāṃ ca puṣpāni svavad ādadīta phalāni cāparivṛtānām* 'He may take, as his own, grass for a cow, and fuel for his fire, as well as the flowers of creepers and trees and their fruit, *if they be unenclosed*' (tr. Bühler). In other words, given a minus specification of the feature 'enclosure', removal of the articles enumerated does not constitute theft. Similarly, in the law relative to trespass and property damage by domestic animals, responsibility falls on the owner or herdsman (Gautama XII 19–20). But the law further specifies (XII 21): *pathi kṣetre 'nāvṛte pālakṣetrikayoḥ* '(If the damage was done) in an unenclosed field near the road, (the responsibility falls) on the herdsman and on the owner of the field' (tr. Bühler).

For the feature of *outcry* in Hindu law, we may refer to the numerous textual citations and references in the study of W. Schulze noted above. One example may suffice, from the Rigveda (4.38.5): *enáṃ vastramáthiṃ ná tāyúm ánu krośanti kṣitáyaḥ* 'The inhabitants cry out to him as to a clothes-stealing thief'. We have here the earliest instance of the verb (*anu-*) *kruś-* which Schulze convincingly connects with OEng. *hréam* '(juridical) outcry'. For the feature of *tracking* and house-search ('Spurfolge' and

'Haussuchung'), see Schulze, *op. cit.* 168 and 188; for Pūṣan the patron deity of tracking, see section four below.

We have already seen the Greek opposition *klopḗ/harpagḗ*, 'theft'/'robbery', which corresponds exactly to the Latin *furtum/rapina*. The opposition *furtum manifestum/f. nec manifestum* is likewise attested in Greek; cf. the expression *ep'autophórōi labeîn* 'catch in the act' (a derivative of *phór* 'thief', *phōrá* 'theft').

The search for stolen goods, tracking and house-search, has its proper technical term, *phōráō*. In Greek law as well, the searcher was obliged to enter the house naked, or clad only in an undergarment. Cf. Aristophanes, *Nubes* 498–9: *gumnoùs eisiénai nomízetai. all'oukhì phōrásōn égōg'eisérkhomai.* The same proviso recurs in Plato's Laws, 954a: *gumnòs è̀ khitōnískon ékhōn ázōstos . . . hoútō phōrân.*[34]

The distinctive feature of *status* is likewise present in Greek; the opposition *slave (doûlos)/non-slave* runs through the whole legal system, as in the institution of noxality discussed earlier.

For the opposition *night/day*, and the right to slay the nocturnal thief as an exercise of private justice, we may quote the law of Solon cited by Demosthenes, *c. Timocr.* 113, pl. 736: *ei mén tis meth'hēméran hupèr pentékonta drakhmàs kléptoi, apagōgèn pròs toùs héndek'eînai; ei dé tis núktōr hotioûn kléptoi, toûton exeînai kaì apokteînai kaì trôsai diókonta kaì apagageîn toîs héndeka, ei boúloito.*

This law is Indo-European in substance; the Solonian innovation is doubtless solely in introducing the *option* of handing over the nocturnal thief to the public authority: characteristically an early step in the extension of public authority and the concomitant restriction of the area of private justice.

In the law of Gortyna (which has nothing to say about theft) we have also the opposition *night/day* in the law of rape, where it is unknown in Attic law: (II.13–15) *ai de ka dedamnamenan ped'ameran, odelon, ai de k'en nutti, du'odelons.*[35]

In the Twelve Tables, it is unlawful to kill the thief by day unless he offers armed resistance, *nisi se telo defendat*; this clause evidently formed part of the article beginning *si nox furtum faxsit.* In Greek we have the same proviso, in the law reported by Demosthenes, *c. Aristocr.* 60, p. 639: *kaì eàn phéronta è̀ ágonta bíai adíkōs euthùs amunómenos kteínēi, nēpoinèi tethnánai.*

In Greek law just as in Roman law, if the feature *night* is specified as 'no', then the feature *violence* must be specified as 'yes' if the thief is to be rightly slain. Together these two exactly recover the legal system of articles VIII.12 and 13 of the Twelve Tables, and it is beyond question that the two Greek statutes simply cover two aspects of a unitary law of theft.[36]

For the remaining features of the Greek law of theft we may—pursuing a fruitful suggestion of W. Schulze, *loc. cit.* 184—examine a passage of Euripides' *Trojan Women*, 998ff. Hecuba bitterly attacks Helen, and her impassioned speech springs from a sense of legal, not merely moral outrage. The operative word appears in her conclusion (1031–2) when she implores

Menelaus to slay Helen: *n ó m o n dè taîs állaisi thès / gunaixí, thnḗiskein hḗtis àn prodôi pósin.* Hecuba has scornfully asked Helen (998ff.):

Bíai gàr paîda phēis s'ágein emón; tís Spartiatŏn ḗisthet'? è poían boễn anōlóluxas? . . .

epeì dè Troían ễlthes Argeîoí té sou kat'íkhnos . . .

This passage is impregnated with technical legal terms: *ágein* the stealing, *bía* the violence, *boḗ* the outcry, *íkhnos* the tracking. We have in these few poetic lines a specification of four juridical distinctive features. Taken in isolation, each of these terms is only a lexical item, and one common enough in Euripides. But here they are structurally interrelated and sequentially ordered to form a legal system. It is this total legal system which Euripides makes Hecuba invoke. For had Paris stolen (*ágein*) Helen by force (*bía*), had Helen made an outcry (*boḗ*), it would have been Paris who could be rightly slain (*nēpoineì tethnánai*, supra). But it is Paris' innocence and Helen's guilt which Hecuba asserts; and the inexorable legal consequence is that Helen should be rightly slain (*thnḗiskein*).

IV. An Indo-European God of Exchange and Reciprocity?

A discussion of theft would not be complete without a reference to the patron god of thieves, Hermes-Mercury in the Greco-Roman tradition. Hermes is a complex and interesting figure, and his patronage of theft is only one aspect of a more general function; see the detailed article of Eitrem in Pauly-Wissowa *RE s.v. Hermes* (henceforth cited as Eitrem). If we were to attempt to characterize that function by a single word, we would be inclined to term him the patron god of the fundamental anthropological and sociological principle of *reciprocity*, as promulgated in Mauss' *Essai sur le don* (cf. p. 20 above).

On the one hand, he presides over the circulation of goods and wealth, of exchange: thus Hermes *Agoraîos*, *Empolaîos*, *kerdôos* (Eitrem, col. 755–6); *Mercurius nundinator*, *negotiator* (Dessau 3202, 3201), and the possible connection of his Latin name (via Etruscan?) with *merx*. On the other hand, he is called in an inscription (Dessau 3200) *sermonis dator*; cf. his Greek epithet *lógios*, which surely figures the exchange inherent in the speech situation, in discourse, rather than being merely derivative from the herald's strong voice (so Eitrem col. 781). It is remarkable that the characteristic actions of Hermes are all reversible, or go in two directions: he gives sleep—and takes it away; he conducts the dead from the earth to the underworld—and back again. Thus Vergil, *Aen.* 4.242–4:

tum virgam capit; hac animas ille evocat Orco/pallentis, alias sub Tartara tristia mittit,/dat somnos adimitque, et lumina morte resignat.

He is the go-between, the *mediator* between the earth and the underworld, between heaven and earth, between the sphere of the divine and the sphere of mortal men. It is this function as intermediary which is paramount in Hermes as the 'messenger of the gods', *ággelos athanátōn*, *kêrux theōn*, *Iovis qui nuntius perhibetur*. As the envoy of Zeus, Hermes is the intermediary, as it were, from up to down. But this is only one aspect of the total

notion; the direction is reversed (movement down to up) in Hermes' function as announcer of the sacrifice TO the gods, which appears in the archaic formula (with the iteration characteristic of ritual language) *spondḗ spondḗ* uttered by Hermes in Aristophanes, *Pax* 433.[37]

As the mediating figure between two points, it is not surprising to find him closely associated with roads, and protecting travelers. We have the epithets *hódios* and *enódios*, and in particular the ubiquitous herms (*hermaî*) which mark the way. Priam meeting Hermes (not recognizing him) terms him a *hodoipóros* 'wayfarer' at Ω 375. Here the opposition of directionality (up/down) is neutralized.

Hermes as the patron god of thieves may be looked upon as presiding over the circulation of wealth *in malam partem*; he is similarly *dólios* as well as *lógios* in the exchange of words. But in complete agreement with the reciprocal, reversible character of his attributes, Hermes is also *mastḗrios*, the patron of tracking: Aeschylus, *Suppl.* 920 *Hermêi megístōi proxénōi mastēríōi*. Cf. Dindorf ad loc.: 'Mercurius dicebatur *mast.*, quatenus res amissas recuperare, et ubi delitescerent inquirere credebatur. SCH.' Hermes is thus both the prince of thieves (*philētéōn órkhamos*) and the discoverer of theft (*mastḗrios*); again a reciprocal relation. Nowhere is this more clearly set forth than in the Homeric Hymn to Hermes. The episodes of the stealing of Apollo's cattle by Hermes and of the tracking of the cattle by Apollo form an antithetic couple, together recovering two aspects of the unitary function of the god Hermes to whom—not to Apollo—the hymn is composed. This symbolic representation in the sphere of myth is yet another justification for Mauss' characterization of the system of reciprocity as *un phénomène social 'total'*.

The figure of Hermes *mastḗrios* permits also a comparison further afield, with Vedic mythology. Wilhelm Schulze has pointed out (*Kl. Schriften* 188n1), in connection with tracking but without specifically comparing Hermes *mastḗrios* (noted *ibid.* 168n 3), that there is a Vedic patron divinity of tracking, Pūṣan. His function is delineated in RV 6.54; cf. line 7 *pūṣā́ gā́ ánu etu naḥ* 'let Pūṣan go after our cattle'. It is worthwhile dwelling briefly on the figures of Hermes and Pūṣan, for their similarity is quite systematic; just as Hermes, so Pūṣan incarnates the notion of exchange and reciprocity.[38]

Hermes as *agoraîos, empolaîos* presides over the circulation of wealth. So Pūṣan is *paśupā́* 'protector of wealth', (6.58.2) *ánaṣṭavedas* 'he whose property is not lost' (10.17.3),[39] as well as presiding precisely over the recovery of lost, strayed, or stolen property.

We have noted the symbolism of Hermes and the road, as *enódios*. In RV 6.53.1 Pūṣan is called *pathas pate* (voc.) 'master of the way'; we have an archaic collocation *pathas-pati-* like IE **dems-poti-*. The parallelism with Gk. *hodoi-póros*, likewise an old collocation with a locative, is noteworthy. He is implored for the protection of travelers, RV 1.42.2–3, and in general this aspect of Pūṣan is well marked in the Rigveda and later Indic tradition.

Just as the road symbolizes the notion of reciprocity, so do doors. Thus Hermes assures the protection of the threshold, doors, and even hinges; cf.

his cultic epithets *propúlaios*, *púlios*, *thuraîos*, and *strophaîos* (*stróphigges* 'hinges'). Macdonell notes from the House-Sūtra literature (ŚGS 2.14.9) that in the morning and evening offerings to all gods and beings, Pūṣan receives his on the threshold of the house.

Hermes is regularly depicted in Greek literature and art as having alternatively a herald's staff (*kērukeîon*), a scepter (*skēptron* B 103), a whip for driving cattle (*mástiga phaeinḗn/boukolías Hymn Herm.* 497), or a golden wand with magical powers to put to sleep or awaken the sleeping (Ω 343–4, cf. *Hymn Herm.* 529–30):

héileto dè rhábdon, têi t'andrôn ómmata thélgei/hôn ethélei, toùs d'aûte
kaì hupnóontas egeírei

Pūṣan in the Rigveda has alternatively a golden axe (*hiraṇyavāśīmattama* 1.42.6), a goad (*áṣṭrā* 6.53.9), or a similar instrument (*árā* 6.53.8 cf. Geldner *ad loc.*) which has the specific property of arousing *brahma*: *yắm pūṣan brahmacódanīm árām bibharṣi*. Compare also his epithet *dhiyaṃjinvá* 'Gedankenwecker' at 1.89.5, 6.58.2, 10.26.4. In both the Hellenic and the Vedic deity this symbolic attribute is doubtless phallic. For Hermes this is above all certain from his most archaic representation, the herm, 'deren wichtigstes Kennzeichen das männliche Glied war' (Eitrem, col. 774). In the case of Pūṣan we may note that the verb *codayati* 'arouse' underlying the epithet *brahmacodanī* of his *ārā* 'goad' is the same as that used of the male organ in the graphic mantra 10.101.12:

kápṛn nárah kapṛthám úd dadhātana codáyata khudáta[40] *vájasātaye*
'Das Glied, ihr Männer, das Glied richtet auf, machet fix, stosset zu, um den Preis zugewinnen'

Exactly like Hermes, Pūṣan is the go-between, the mediator, the intermediary. His boats traverse the space between Heaven and Earth (*antárikṣe caranti*) and with them he acts as messenger of the sun (*dūtyắm yắsi* 6.58.3), though normally the messenger proper in the Rigveda is Agni.[41] But it is Pūṣan who conducts the dead to their abode, just as Hermes does (first at *Od.* 24.1–5, of the suitors of Penelope). In 10.17.3 (the lines are addressed to the dead man): *pūṣắ tvetáś cyāvayatu . . . sá tvaitébhyaḥ pári dadat pitṛ́bhyo* 'Pūṣan soll dich von hier befördern . . . Er übergäbe dich diesen Vätern' (together with Agni and Savitṛ).

We have noted the two-way character of Hermes' characteristic actions which symbolically represents his basic function of incarnating the notion of reciprocity. He gives and takes away, goes and comes: moving between in either direction, he is the link between the poles of earth and heaven, between the living and the dead, between gods and men, whether as messenger, guide, or companion. Just so Pūṣan in the final mantra of the cultic episode just quoted, 10.17.6:

prápathe pathắm ajaniṣṭa pūṣắ prápathe diváḥ prápathe pṛthivyắḥ /ubhé
abhí priyátame sadhásthe ắ ca párā ca carati prajānán
'On the forward road of the roads hath Pūṣan been born, on the forward road of heaven, on the forward road of the earth; unto both the dearest stations, both hither and yon, goeth he, foreknowing'[42]

Hermes is the mediator, the communication link between gods and men

not only from up to down (*ággelos athanátōn*) but from down to up, in his function as sacrificer (Eitrem, col. 779) and announcer of the sacrifice. In the latter we have an exactly comparable function of Pūṣan. In the aśvamedha hymn 1.162.4 the sacrificial horse is led around the place of sacrifice, preceded by a goat identified as Pūṣan's; the goat (likewise sacrificed) has the specific function of anouncing the sacrifice to the gods:

átrā pūṣṇáḥ prathamó bhāgá eti yajñáṁ devébhyaḥ prativedáyann ajáḥ
'so geht dabei das Opferanteil des Pūṣan voran, der Bock, der den Göttern das Opfer meldet'

The goat is elsewhere an attribute of Pūṣan; his chariot is drawn by goats instead of horses, and is contrasted with Indra in 6.57.3. Whether there is a specific connection with the goat as an attribute of Hermes (along with other animals, cf. Eitrem, col. 757–9) is uncertain; but the annunciatory function is clearly common to both traditions.[43]

In the Vedic passage just quoted, the verb used for 'announcing' is the participle *prati-vedáyant-*, an iterative-causative **u̯oid-eie-* to the root *u̯eid-*. This form has an exact cognate in Old Irish *foídid* 'sends' (**u̯oid-eie-*), and the equation is the more probative for the derivational isolation of this verb in Irish, and the relative rarity of the *o*-grade iterative-causative type in Celtic in general. From this verb we have formed the agent noun *foídem* (archaic *foídïam*) 'messenger'. It is worth noting that in the oldest Irish text that we possess, the late sixth century eulogy of St. Columba, this noun occurs in the following context (LU 596): *ardon bath bá fiadat foídiam* 'he has died (on us *vel sim.*) who was the messenger of the Lord', which the later native glossator rendered as *atbath erund intí nod faidmis co ar Fiadat* 'he has died on us whom we used to send to our Lord'. It is possible to interpret this in purely Christian terms, with the earlier revelatory function (*fíadat foídam*) of the saint later rendered as an intercessory function (*intí nod faidmis co ar F.*). But the etymological connections of *foídiam* and the fact that the Irish name of the Lord itself is clearly a pre-Christian divine name or epithet (from the same root, **u̯eid-ont-* 'seeing, knowing', and cf. the dwarf *Alviss* in Norse mythology) would at least suggest the possibility that the representation of St. Columba as the 'messenger of the Lord' (*fíadat foídiam*) transposes a pre-Christian Celtic mythological figure entirely similar in function to Hermes.

There is one final and striking point of comparison between Hermes and Pūṣan. Hermes has in Greek literature the function of leading the bride to the groom, *numphagōgós* (Eitrem co. 774–5). So in the Homeric Hymn to Aphrodite, Hermes allegedly brings Aphrodite to Anchises (117ff.):

énthen m'hḗrpaxe khrusórrapis Argeiphóntēs . . . /Ankhíseō dé me pháske paraì lékhesin kaléesthai/kouridíēn álokhon, soì d' aglaà tékna tekeîsthai.

The same situation is comically depicted in Aristophanes' *Pax* 706ff., where Hermes gives the bride away with the formula *íthi nun epì toútois tḕn Opṓran lámbane/gunaîka sautōi tḗnde* and then bids them raise not children but grapes (*ekpoioû sautôi bótrus*). In the Vedic figure of Pūṣan we find the same function, even more clearly delineated as bidirectional. In the

wedding-hymn 10.85 ('eine Sammlung von Hochzeitssprüchen', Geldner *ad loc.*) the spokesman for the bride's household invokes Pūṣan to carry her away, *out of the old home*: *pūṣā́ tvetó nayatu hastagŕhya* (26) 'Pūṣan soll dich von hinnen führen, deine Hand fassend', where we may note the close parallelism with the phraseology of the hymn cited above invoking Pūṣan to carry away the dead, *pūṣā́ tvetáś cyāvayatu*; the distinction is clearly marked in the wedding ceremony by the use of the particular verb *nayati* '(uxorem) ducit', on which see Benveniste, *Hittite et indo-européen* 33ff. But in verse 37 of the same wedding hymn, at the ceremony of the grasping of the bride's hand by the groom (*gŕbhṇā́mi te . . . hástam* 'I grasp thy hand'), it is the groom who invokes Pūṣan to carry her *into the new home*: *tā́m pūṣañ chivátamām érayasva*. Thus Pūṣan assures both the going out of the bride (from her natal household) and her coming in (to her marital household). Another instance of the latter function in the Rigveda, from an earlier book, is 9.67.10, when Pūṣan is called upon to provide the singers with maidens, i.e. from outside: *pūṣā́/ā́ bhakṣat kanyàsu naḥ* 'Pūṣan verhelfe uns zu Jungfrauen'.

It is this function of the two divinities, clearly present in the figure of Hermes and more specifically described in the role of Pūṣan, which provides the conclusive proof of their basic attribute as incarnators of the principle of exchange and reciprocity. For Lévi-Strauss has shown at length in his work *Les structures élémentaires de la parenté* that perhaps the fundamental principle regulating human society is the circulation of women, according to the system of exchange and reciprocity: women are given out of the family in order that others come in in return—and vice versa.

Hermes and Pūṣan thus preside over both the physiological and the economic well-being of society, by assuring both the circulation of women and the circulation of wealth, through the notion of exchange. Both divinities symbolize the relation of reciprocity, bidirectionality, by their association with the road; both are the mediators, who assure the communication in either direction between gods and men, heaven and earth, or the living and the dead. Finally Hermes assures likewise the circulation of speech, communication proper; this function is not associated with Pūṣan, but perhaps only because in the Rigveda there a special goddess *Vāc*, the divinized 'Speech'. As Hermes gave speech to men (*sermonis dator*), so did Vāc: 8.100.10 *yad vā́g vádanty avicetanā́ni . . . niṣasā́da* 'Als die sprechende Rede . . . sich bei den unvernünftigen niederliess'. In the hymn to Vāc 10.125 the goddess vaunts her annunciatory and mediatory function in terms that strikingly recall the descriptions of Pūṣan: *ahám dyā́vāpŕthivī ā́ viveśa* (6) 'ich durchdringe Himmel und Erde', cf. 10.17.6 cited above. Hence it is possible that the figure of Vāc embodies a function that was once another aspect of Pūṣan; the transparent name would argue for this as well.

As mentioned above, Wilhelm Schulze noted at two points in the same article Hermes as *mastḗrios* and Pūṣan as 'Gott der Spurfolge', yet forebore to compare the two figures directly, as we have done. Perhaps this is to be explained by Schulze's etymological equation of *Pūṣan* with the Greek god

Pán, especially in the Arcadian form *Paōn*, *Kleine Schriften* 217. But in Greek tradition Hermes is the father of Pan (*Hermeíao gónos* Hymn. Hom. Pan 1). In the archaic Arcadian figures of Pan transformed into Hermes = Pūṣan, we have to deal with the reflexes of an Indo-European divinity reconstructible both in name and in function. A certain haziness in the form is in a divine name to be expected, because of the factor of tabu-deformation: Schulze noted the relation between *uṣas* and *ēōs*, Dor. *awōs*, and we may further cite the Lithuanian deity *Perkūnas* with long *ū*, beside the Slavic *Perunъ* < **per(k)aunos*, with the dipthong appearing in Gk. *keraunós* (from metathesized **ker(p)aunos*).

Notes

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[2] Die Quellen des römischen Rechts (*Öster. Akad. Wiss., Denkschriften* Bd. 2) 10 (Wien, 1963). The italics are mine.

[3] R. von Ihering, *Vorgeschichte der Indoeuropäer* 357 (Leipzig, 1894 [from his Nachlass]): 'Versetzen wir uns im Geist in die Zeit zurück, als die Arier, nachdem die Auswanderungsfrage in Princip entschieden war, die genaueren Modalitäten der Auswanderung berieten. Wann soll man aufbrechen? Im Winter? da ist es noch zu kalt, wir wissen, dass auch die Arier den Winter schwer empfanden. Im Sommer (unserm Sommer und Herbst)? da ist es zu heiss. So erübrigt sich nichts als der Frühling, da ist es weder zu heiss, noch zu kalt, es herrscht eine milde Witterung, welche das Marschieren ohne alle Beschwerde möglich macht. Im Frühling und zwar noch der römischen Tradition genauer gesprochen: am ersten März haben unsere Vorfahren ihre Heimat verlassen.'

[4] So C. W. Westrup, Recherches sur les formes antiques de mariage dans l'ancien droit romain (*KDVS. hist. -fil. medd.* 30.1) p. 69 n3 (Copenhagen, 1943); Th. Mayer-Maly, *Pauly-Wissowa, RE*, 2. Reihe 17.1132.

[5] Cf. E. Hermann, Die Eheformen der Urindogermanen 46–8 (*Nachrichten Gesell. Wiss. Göttingen* N.F. 1.2 [1934]). *Raubehe* is certainly not confined to the Indo-European tradition alone. For the practice in present-day Turkey, for example, see the study of J. Cuisenier in *L'homme* 4.80–1 (1964), with the delightful illustration from a contemporary text. But it should be noted that the social status of *Raubehe* and of those who practice it is fundamentally different in Turkish and the given early Indo-European society; cf. note 7 below. The structural integration of the same institution is thus not parallel in the two cultures, and it is that which would have to be the primary datum for any application of, or evaluation of the results of, the comparative method.

[6] *Cáin Lánamna* §§4, 34, in R. Thurneysen, *Studies in Early Irish Law* (Dublin, 1936).

[7] J. Friedrich, *Die hethitischen Gesetze* I §37 (Leiden, 1959). I have adopted the reading of the oldest manuscript A, written in the Old Hittite ductus; see H. G. Güterbock, JCS 15.62–78 (1961). Friedrich may be correct in suggesting that the formula implies that the abductor(s) became outlawed. In any case, a legal interpretation of this passage is to be preferred to V. V. Ivanov's recent suggestion that it is a reminiscence of totemic lycanthropy, *Xettskij jazyk* 23 (Moscow, 1963). Marriage

by abduction is particularly associated with the *kṣatriyas*, the warrior class in Hindu law; Manu III 24 *rākṣaṣam kṣatriyasya ekam* 'the *rākṣasa* (rite) belongs alone to the *kṣatriya*'. The association of the wolf with warrior is common in archaic Irish; cf. the epithets *caīnḟáel* 'fair wolf' in Meyer, *Aelt. ir. Dicht.* 2.23, *delb canann* 'form of a wolf-cub', Meyer, *Bruchstücke* 21. The Hittite URBARRA-*aš* is thus directly in accord with this tradition.

[8] So Friedrich, after Hrozny, *Code hittite* §37, p. 28n6; but more likely dittographic *appan⟨an⟩da*, cf. EGIR-*anda* in B.

[9] The text is that of M. David and H. L. W. Nelson. (Leiden, 1954).

[10] The restoration of *usus* here is certain, cf. David and Nelson's commentary *ad loc.*

[11] *Lectures on the Early History of Institutions* 315–16 (London, 1875).

[12] The emphasis is mine.

[13] E. Benveniste, *Noms d'agent et noms d'action en indo-européen* 97 (Paris, 1948). Note the contrast between the 'subjective', 'internal' *-tu-* in *ūsus*, and the 'objective', 'external' *-ti-* (or its equivalent) in *ūsūcapiō*, *ūsurpatiō*.

[14] Examples from both literary and scientific contemporary English may be found in K. Vonnegut, Jr., *God Bless You, Mr. Rosewater, or Pearls before Swine*, Chapter 2 (New York, 1965), and W. P. Lehmann, *Historical Linguistics* 101 (New York, 1962).

[15] Probably to be corrected to *noxiam*. Cf. Gaius, *Dig.* 47.9.9 *noxiam sarcire*, where the original *noxia* of F was 'corrected' to *noxa* by F^b.

[16] *L'année sociologique* 3e série (1948–49), 7–20.

[17] Cf. the formula *furto noxaque solutus* in the transfer of slaves, which must refer directly to the article *si servus furtum faxit noxiamve noxit* of the Twelve Tables.

[18] Benveniste points out specifically (pp. 19–20) that 'Chez Homère, *alphánō* signifie certes "procurer un gain", mais ce sens est lié à une situation bien définie; le gain en question est celui qu'un captif rapporte à celui qui le vend." Further, 'La "valeur" se caractérise, dans son expression ancienne, comme une 'valeur d'échange' au sens le plus matériel. C'est la valeur d'échange que possède un corps humain qu'on livre pour un certain prix.' It is this fact which completely confirms the etymology of Old Irish *cimbid*, g. *cimbetho* (*i*-stem) 'captive': a Celtic **kṃb-i-ati-* to the root of Gallo-Latin *cambio*, *cambiāre* 'exchange'. The importance of the notion of ransom and exchange in early Celtic society is likewise shown in the Middle Welsh legal technical term *lleydyr gwerth* 'a thief that could redeem himself by paying' (T. Lewis, *A Glossary of Medieval Welsh Law* s.v. *guerth*) in the *Llyfr Blegywryd* 39.8, 122.15 (eds. Stephen J. Williams and J. Enoch Powell, Caerdydd, 1961). Cf. also D. A. Binchy's contribution to this conference on the role of the personal surety.

[19] H. Pedersen, *Hittitisch u. d. anderen indoeur. Spr.* 145; J. Friedrich, Heth. Wb. 187; E. H. Sturtevant, *Comp. Gram of the Hittite lang*^{2} 127; Walde-Hofmann, *Lat. etym. Wb.* s.v. *sarciō*; J. Pokorny, *Idg. etym Wb.* s.v. **serk-*; Hj. Frisk, *Griech. etym. Wb.* s.v. *hérkos*. Vjač. Vs. Ivanov, *Obščeindoevropejskaja, praslavjanskaja, i anatolijskaja jazykovye sistemy* 177 (Moskva, 1965), raises the further possibility of connecting *šarnikzi* with Hitt. *šark-* 'rise', *šer*, *šarra* 'up', as well as with *sarcīre*. (His *sparsī* is presumably a lapsus for *sarxī*, cf. the index, p. 296). But the semantics seem far-fetched. Hitt. *šark-* is synchronically unsegmentable, cf. the old adjectival derivative *šark-u-* 'mighty < lofty'; the derivational relation and the semantic field are identical to those of the Hittite verb *park-* 'rise (mid.)' to *park-u-* 'lofty, high'. On these archaic deverbative adjectives in *-u-* (which are basic to the formation of the Hittite factitive-causative verbal suffix *-n-u-*) cf. also The origin of the *f*-future, *Ériu* 20 (1966).

[20] In Hittite legal language the genitive of the word for 'theft', *tayazil-aš*, is used both as subject and object of a verb, in the meanings 'he of the theft = the thief', 'it of the theft = the compensation' (I §73, 94, 95, of Friedrich *ad loc.*). In my opinion this considerably strengthens Thurneysen's brilliant etymology of Latin *reus*, indifferently 'plaintiff' or 'defendant' at the time of the Twelve Tables, as the old genitive of *rēs*: 'der des Prozesses' (*IF* 14. 131 [1903]). Szemerényi's alternative explanation of

reus, *KZ* 73.179–81 (1956), I find less convincing. The construction with the genitive is probably Indo-European in date, and doubtless underlies such developments as Greek *tà tôn polémōn* (Thuc. 2.11.4), formally posterior to the creation of a definite article. The defining genitive in *urbs Romae*, *Ilíou pólis*, or *hérkos odóntōn* ('the barrier—that of the teeth') is likewise to be so explained. A similar construction exists in Akkadian with *ša* + gen. of the infinitive: Hammurabi §133a *ina bītišu ša akālim ibaššī* 'in his house "such of eating" (subject) existed; §8.67 *ša nadānim la išū* 'such of paying" (object) he does not have'. But while this might have been a factor in the maintenance of the construction in Hittite, it cannot explain its genesis. One may note also that while the defining genitive exists also in Akkadian (*māt šumerim* 'land of Sumer') the 'such of' construction is not found with the genitive without *ša*. Hence the function of the genitive is not exactly comparable in the two families. Cf. H. Kronasser, *Etym. d. Heth. Spr.* §171–2, but who argues unconvincingly for Akkadian origin, and in my opinion lumps together wholly different Hittite constructions.

[21] The archaic construction of A with possessive pronoun suffixed to the preverb, *šer-šit* (*-wa*) *šarnikzi*, is particularly noteworthy, beside the more 'modern' preverb + enclitic pronominal dative *šer* (*-wa*)*-ši šarnikzi* in B. The importance of the former type for Indo-European syntax cannot be overestimated; it is now fully exploited in Vjač. Vs. Ivanov, *Obščeindoevropejskaja, praslavjanskaja, i anatolijskaja jazykovye sistemy* 194–227 (Moskva, 1965).

[22] I have noted in *Celtica* 6.230 n.1 (1963) the striking parallel in legal phraseology between Hitt. *mekki/tepu-še išḫiyanzi* with Old Irish *naiscid . . . for nech* 'binds (an obligation) on someone'; cf. also Latin *nexum*, *obligātiō*.

[23] Ed. Götze, *Kleinas. Forsch.* 1.161ff. (1929).

[24] Thus correct Friedrich, Heth. Wb., s.v. *šarnikzi*. Similarly we have *mān BEL-ŠU šer UL šarnikzi* in the treaty fragment KUB VIII 81 (III 3, 5) edited by Petschow (*loc. cit.*). When *šarnikzi* occurs without the preverb, as in § 95 and § 99 of the Code, or in the texts edited by Petschow, it always resumes an occurrence with preverb in a preceding clause or sentence. This construction is Indo-European in date; see in greater detail my study in *Harv. Stud. in Class. Phil.* 71.115–9 (1966).

[25] For a discussion of the question in the literature before 1959, see Friedrich, Heth. *Gesetze* 88–90; later references in Heth. *Wb. Ergänzunsheft* 2 (1961) s.v. *parn-*. Add E. A. Menabde's study in *Peredneaziatskij Sbornik* p. 22n 30; Menabde's interpretation is very similar—though for different reasons—to that suggested here.

[26] This is not to say that *sarcīre*/*šarnikzi* represent the only Indo-European expression for the notion. We have the root of Gk. *tínō*, *poinḗ*, Skt. *cáyate*, Av. *čikayat* 'he shall atone' (cf. *raēšam čikayat*, virtually identical to *noxiam sarcire*); Lith. *káina*; OCS *cěna* and *kajati sę*; and OIr. *cin* 'liability' < **k*^{w}*inut-s*. In the last the identity of **k*^{w}*inu-*(*t-*) with Gk. *tinu-*makes it evident that we have the direct suffixation of-*t-* onto the present stem with suffix *-nu*. Exactly the same applies to Hitt. *šarnikzel*, where *-zel* < **-tēl* (cf. Lat. *-tēla*) has been added directly to the present stem with nasal infix *šar-ni-k-*. This makes it most likely that Celtic as well once had a verb **k*^{w}*inu-ti* 'pays compensation, atones, discharges liability'. Hence we have a large Eastern area with **k*^{w}*ei-* (Armenian alone not being represented), together with (Goidelic) Celtic—but not Italic, n.b.

[27] R. Haase, *Einführung in das Studium keilschriftlicher Rechtsquellen* (Wiesbaden, 1965).

[28] Although the Laws represent an idealized conception, and as such are not direct evidence, it is still less than credible that Plato would have invented out of whole cloth a system of noxality identical with that of Rome and Hattušaš alike.

[29] E. Norden, *Aus altrömischen Priesterbüchern* 254ff. (Lund, 1939).

[30] Cf. Jolowicz, *An Historical Introduction to the Study of Roman Law*^{3} 139ff. (Cambridge, 1965).

[31] The opposition *manifest theft/not manifest theft* is clear in the eleventh-century *Russian Law*; as in Roman law, the thief who is not caught in the act is subject to restitution and fine, and contrary to the manifest thief, may not be executed. We have

in the older short version of the Russkaja Pravda, in the Academy manuscript ed. B. D. Grekov (Moscow, 1940):

§ 13. *ašče poimetъ kto čjuzъ konь, ljubo oružije, ljubo portъ, a poznajetъ vъ svojemъ miru, to vzjati jemu svoje, a* 3 *grivně za obidu*

§ 14. *ašče poznajetъ kto, ne emletъ jego, to ne rci jemu 'moje', nъ rci jemu tako: 'poidi na svodъ gdě jesi vzjalъ' ili ne poidetъ, to poručnika za piatь dnii*

'If anyone takes another's horse, or weapon, or clothes, and the owner identifies the object within his township, he receives it back and 3 *grivně* for the offense'

'If the owner identifies his property outside of his town he must not seize it outright; do not tell the man who holds the property "this is mine," but tell him thus: "come for confrontment to the place where you got it"; and if he does not come immediately he must produce two bails to guarantee that he will come within five days.' (tr. G. Vernadsky, *Medieval Russian Laws* (New York, 1947).

[32] See in general the study of E. Weiss, *'lance et licio'*, *ZSS* 43.455–65. But in the interpretation of *licium* as 'tether, rope' rather than the traditional 'loincloth', I follow F. Wieacker in his excellent study, Zwölftafelprobleme, *RIDI* n.s. 3.459–91 (1956). A 'tether' would accord more with the apparent meaning of the rare word *licium* in other contexts; the meaning 'loincloth' is a rationalization of the classical Roman jurists, who dimly recalled the proviso of nakedness, but for whom the ritual house-search itself had already long since fallen into desuetude.

[33] Cf. *Les structures élémentaires de la parenté* 177 (Paris, 1949).

[34] For the institutions of tracking (Russ. *sled*) and house-search (Russ. *obysk v dome*) in Slavic, see the old study of H. Jireček, *Über Eigentumsverletzungen und deren Rechtsfolgen nach dem altböhmischen Rechte* (Vienna, 1855). Our earliest detailed information would appear to be the Byelorussian *Statut velikogo knjažestva litovskogo* of 1529 (ed. with Russian translation by K. I. Jablonskis [Minsk, 1960]), where the technical term for house-searching is *tresti dom*. Jireček states that the searcher must remove his outer garment, and this Slavic proviso of quasi-nakedness, which would accord very nicely with Greek, Roman, and Germanic practice, has been repeated in studies of historical jurisprudence. Unfortunately, the Slavic law texts themselves do not mention any such proviso that I have been able to discover; but it may well appear in folkloristic texts.

[35] See the edition of Bücheler and Zitelmann, *Rhein. Mus.* 40, Ergänzungsheft (1885), with the latter's juridical commentary (p. 102).

[36] These features are likewise distinctive in Slavic law in the case of manifest theft. The relevant articles in the short version of the Russkaja Pravda are as follows: § 38 *ašče ubьjutъ tatja na svojemъ dvorě, ljubo u klěti ili u xlěva, to toji ubitъ; ašče li do světa deržatъ, to vesti jego na knjažъ dvorъ; a ože li ubьjutъ, a ljudi budutъ viděli svjazanъ, to platiti v nemъ*. 'If they kill a thief in their own yard, or at the barn, or at the stable, he is [rightly] killed; but if they hold him until daylight, they have to bring him to the prince's court; and in case they hold him until daylight and then kill him, and people have seen him bound [before he was killed], they have to pay for him'. (tr. Vernadsky). The passage does, however, bear a certain resemblance to the Mosaic law (Exodus 22.2–3), as noted by V. O. Ključevskij in his discussion of the Russkaja Pravda, *Kurs russkoj istorii*, čast' 1, lekcija 13 (*Sočinenija* 1.210 [Moskva, 1956]), and the possibility of diffusion cannot be ignored. § 21 *aže ubijutъ ogniščanina u klěti, ili u konja, ili u govjada, ili u korovьjě tatьby, to ubiti vъ psa město*. 'And if, while stealing cows, they murder the bailiff near a barn, or near a horse[stable] or a cow[shed], [the one who murders the bailiff] is to be killed like a dog' (tr. Vernadsky).

In both articles we have the feature of enclosure, the equivalent of the Latin *inter parietes*: *na svojemъ dvorě* 'in their own yard', *u klěti* 'at or near the barn'. In article 38 the opposition *night/day* is clearly specified by the phrase *do světa* 'until daylight'. And finally, the feature of violence is specified in article 21. In Slavic law the provisions thus are virtually identical with the early Roman and Greek law of manifest theft: the nocturnal thief caught in the act may be slain with impunity; the diurnal thief only if he further commits murder.

The Russian *aščе ubьjutь tatja . . . to toji ubitъ* is remarkably similar to the Latin *si im occisit, iure caesus esto*. Have we to deal with a reminiscence of Indo-European phraseology? On the archaic absence of the preverb in *caesus* = *occīsus* see the forthcoming paper mentioned in note 20 above, with the literature cited. Old Russian *to* in this passage (and commonly elsewhere in the Russkaja Pravda) is a sentence connective, cognate with Old Hittite *ta*, and continuing intact an archaic Indo-European syntactic feature; cf. Ivanov, *Obščeidoevr. . . . sistemy* 188–9.

[37] For the notion of *mediation* and the term or figure which provides for the *communication* between heaven and earth, as well as for the notion of directionality and the opposition *up-down*, cf. Lévi-Strauss, *Le cru et le cuit* 72, 123–4, 141–5, 171, et *passim*. For such oppositions as heaven-earth, earth-underworld, divine-mortal, see also the treatment in Ivanov-Toporov, op. cit. (p. 335 above).

[38] For a concise description of the functions and attributes of Pūṣan, with ample textual references, see Macdonnell, *Vedic Mythology* 35–7; cf. also Hillebrandt, *Vedische Mythologie* (Kleine Ausgabe) 111–115.

[39] On *paśu* 'wealth' *cf.* Benveniste's contribution in the present volume. Here the earlier *ánaṣṭavedas* (Bk. 6) provides the interpretation of the later *ánaṣṭapaśu* (Bk. 10), despite the *bhúvanasya gopāḥ* 'herdsman of the world' which follows the latter.

[40] Note the 'expressive' value of the RV hapax *khudáti* here, phonetically and morphologically a rhyme-word to *tudáti*, in connection with the problem of the Indo-European voiceless aspirates and the well-known views of Meillet.

[41] For other similarities between Hermes and Agni cf. Eitrem, col. 778–9.

[42] Whitney's translation at AV 7.9.1 (Harv. Orient. Ser., vol. 7).

[43] A type of European snipe (scolopax gallinago) is called *Himmelsziege* in German (dial. *Himmelsgeiss*), and in the Baltic lands Lith. *dangaus ožys* 'heaven's goat', *dievo ožys* 'god's goat', and the more striking *Perkuno ožys* 'Perkunas' goat'. Likewise Finn. *taivaanvuohi* 'heaven's goat', and Karel. Finn. *Pyhän Iljan vuohi* 'die Ziege des heiligen Ilja (I. als Bezeichnung des Donners); cf. Perkunas. According to the *Handwörterbuch des deutschen Aberglaubens*, s.v. *Himmelsziege*, this bird is connected with mythology, and its flight announces a coming storm. But the similarity of Pūṣan's goat, who announces the sacrifice, may be mere coincidence.

Celtic Suretyship, A Fossilized Indo-European Institution?

by D. A. Binchy

The very title of this paper is bound to evoke controversy. Legal historians, in particular historians of Roman law, tend to fight shy of the 'comparative' method; indeed their attitude toward it recalls that of the older school of 'classical philologists,' from whom I learned Greek and Latin, toward comparative linguistics. To quite a number of Romanists the adjective 'Indo-European' applied to law and institutions is almost a dirty word. I readily admit that there are genuine reasons for this mistrust: laws and institutions cannot be affiliated to a common ancestor with anything like the same certainty as languages, and the mere fact that the same legal rule is found among most or all of the Indo-European nations does not necessarily prove common origin. Yet, when every allowance has been made for this uncertainty, there is to my mind ample justification for the tentative conclusion reached by one of the greatest of modern Romanists, Leopold Wenger:

> Gewisse Rechtssätze lassen sich als indogermanisches gemeinsames Erbgut ansprechen, das alle oder einige arische Tochtervölker aus einer gemeinsamen Vorzeit in ihre nationale geschichtliche Sonderentwicklung mitgebracht haben.[1]

Another objection from quite a different angle may be urged against my title. Suretyship is an almost universal feature of early society. In 1964, for example, the Société Jean Bodin chose for the theme of its two international congresses at Vienna 'personal securities (*les sécurités personelles*)' in ancient legal systems; papers were submitted on suretyship in Babylon and Assyria, Israel and Japan, China and Indonesia, as well as in Greece, Rome, and the Germanic countries. Faced with such far-reaching distribution, can one safely assume that suretyship in the 'Indo-European' systems is 'gemeinsames Erbgut'? May it not have been evolved independently in each system in accordance with Toynbee's formula of 'challenge and response'—the same challenge evoking a substantially similar response in all societies, Indo-European and non-Indo-European alike? This explanation must always be considered possible, for I know of no way of disproving it. There is, for example, no inherited word for suretyship to buttress the argument that the institution was already in existence during the period of Indo-European unity; the correspondence between Lat. *uas*

(*uadis*) and Gmc. *wed* is hardly sufficient to establish this. On the other hand, we also lack common words for several other things which must have existed before the 'dispersal'.[2] Again, the widespread incidence of suretyship in ancient societies shows that it must have developed at a very early stage in the evolution of every social group; hence why should it not date from the period of unity?

Our modern view of the surety and his functions is almost entirely based on the *fideiussor* of classical Roman law. But he is at once the product of a lengthy evolution and the symbol of a relatively advanced social and economic organization. Behind him, it is true, loom the dim figures of the *uindex*, the *uas* and the *praes*, but these survive in the works of the great jurisconsults only as the ghosts of a vanished order, with the result that in our day scholars are still sharply divided about the interpretation of the few brocards of archaic text concerning them. The trouble is that from purely Roman sources it is impossible to reconstruct the primitive form of society in which the earliest types of surety came into being. Wenger has aptly summarized the successive stages in the evolution of the Roman polity as 'Stammstaat, Stadstaat, Landstaat, Weltstaat'.[3] For the first stage we have no records, for the second very few; and I suggest that some of the gaps in our knowledge may be filled by studying some of the more retarded societies who spoke an Indo-European language. In this paper I propose to deal with one of these societies.

German legal historians divide sureties according to their functions into 'Zahlbürgen' and 'Gestellungsbürgen'. The first is the normal surety of modern times who assumes a solidary obligation to answer for the debt or default of his principal by paying the sum involved out of his own pocket. But it is not sufficiently realized that he meets the needs of the average creditor only in a highly organized society, with a cash economy and a state-administered system of law. In such a society the creditor's main concern is to provide against the principal debtor's being unable to pay his debt for lack of means or (though this is much rarer) against his fraudulently evading it; the contingency of a perfectly solvent debtor who just refuses to pay is now quite unreal. But in earlier times it was an ever-present danger, and the 'Gestellungsbürge', who survives only as the modern 'bail' (or 'bailsman'), represents an attempt to avoid it or at least to minimize it. He warranted that his principal would appear in court to answer any charge (whether arising from contract or from delict) which another party might bring against him; further that he would abide by the decision of the judge or arbitrator. In a society where recourse to a tribunal was not yet enforced by public authority, and where the enforcement of judicial sentences was left altogether to the victorious party and his kindred, the 'Gestellungsbürge' was obviously a key figure in the observance of law and order.

I submit, however, that at a still earlier stage he had an even more vital function. The original surety was not designed merely to supplement or to facilitate the public administration of justice: he was rather a primitive

substitute for it. In a tribal society where recourse to arbitration was still optional and where no machinery existed for the enforcement of the arbitrator's award, the first duty of the surety was to COMPEL the principal to carry out his obligations. One might perhaps call him a 'Leistungsbürge', a surety for performance, who pledged himself to enforce, by all means available to him (including the use of violence), the discharge of his obligations by the principal. Guaranteeing his eventual appearance in court should a suit be brought against him by the other party is merely accessory to the primary function of forcing him to perform his side of the agreement.

The origin of suretyship in primitive society can thus be traced to the absence of any public enforcement of private engagements. Lacking this, each party to a contract[4] would seek some more effective method of binding the other than the mere exchange of solemn formulas in the presence of witnesses. Perhaps the earliest additional security was the invocation by one or both parties of supernatural powers as guarantors of the engagement, an expanded form of the oath which would give these powers a direct interest in the performance of his promise by the party who had invoked them and would expose him to punishment at their hands if he broke it. In early Irish legend there are several examples of what is called the 'suretyship of the elements', in which the sun, the moon, the air, the stars, etc. (all of them objects of worship in pre-Christian Ireland) are 'invoked' to guarantee an agreement. One of them, which brings us to the border of the historical period, relates how Loegaire, king of Tara for some years in the first half of the fifth century, having been captured by the Leinstermen after an unsuccessful invasion of their territory to levy tribute, was subsequently released on giving a solemn promise that he would never again lay claim to the tribute. He invoked as guarantors of this promise 'sun and moon, water and air, day and night, earth and sea'. Two years later he again invaded Laginian territory in a further attempt to levy the tribute, whereupon 'the elements passed a doom of death' on him: the sun burned him, the air departed from him (depriving him of breath) and the earth swallowed him. His alleged fate is commemorated in a frequently cited quatrain:

> Loegaire son of Niall died in the green land on the slope of Caise; the elements of God, whose suretyship he had invoked, passed a doom of death on the king.[5]

This ancient practice was subsequently Christianized, and in place of the old gods the Gospels and the saints are recognized in the Irish laws as valid 'sureties'.[6] I have little doubt that the Welsh *briduw*[7] and the Anglo-Saxon *Godborg*[8] are similar vestigial survivals.

But even in primitive societies men would have noted that some of their fellows occasionally braved the sanctions of a divine or supernatural surety and (unlike Loegaire) escaped unpunished. An analogous, but more tangible, guarantee would be provided if some powerful member of society was 'invoked' in place of the gods to warrant the discharge of an obligation by one of his inferiors. Such a person would be able, not merely to punish an eventual defaulter, but to compel performance, in other words

to do the work discharged by the state in more developed societies. And this, I believe, was the earliest type of human surety, a person who, by reason of his 'superiority' over another, was in a position to force that other to meet his obligations. Whether his 'superiority' was due to his prominent status in the tribal community or to his being the recognized head of the kin-group to which the other person belonged is for the moment immaterial; the essential thing was that he could exercise his authority, and if necessary use force, to compel performance of the obligation. He did not, of course, assume any engagement, either of *Schuld* or *Haftung*, toward the other party to the contract, and in no circumstances did he become liable for the original debt; like the supernatural agencies he had replaced, he was pledged to enforce it, not to discharge it.

From the earliest times a less exalted type of surety may well have existed side by side with the enforcing surety. This was the 'hostage-surety' (*Geiselbürge*). The political authority of the tribal king, and—in Ireland at least—the overlordship of a superior king over several tribal kings, were acknowledged by the presence of hostages whose life and liberty were forfeit if the kindred or the under-king who had given them revolted or refused further allegiance. What was more natural than that this system of securing political fidelity by means of human pledges should be transferred to the domain of private obligations also? The hostage-surety may well have emerged quite independently of the enforcing surety; perhaps he was first used in cases where no 'enforcer' could be found.[9] It may be significant that in ancient Irish law the two types of surety are never found in the same contract. To some extent their functions overlapped: the hostage-surety was widely used to guarantee attendance at court, and a trace of this doubtless survives in English criminal law where the bail for felony are bound 'body for body'.[10] The main difference between them is that of liability (*Haftung*) to the other party: unlike the enforcing surety, the hostage-surety is bound to him by a set of obligations which come into operation when the principal has failed to perform his side of the agreement. Thereupon the surety is 'forfeit' and falls completely into the power of the creditor. Up to this point he could have been 'redeemed', just like an inanimate pledge, by the debtor's discharging his obligations; but to judge from the Irish evidence he could not regain his freedom by paying the debt himself. In other words he was not originally a 'Zahlbürge'.[11]

The appearance of the 'Zahlbürge', who pledges, not his 'honor', like the enforcing surety, nor his person, like the hostage-surety, but his property, is to my mind a feature of a more developed and stable order. The gradual consolidation of state-administered justice made it impossible for a debtor who was solvent to refuse to pay his debt (except by flight from the area of jurisdiction) and thus rendered the enforcing surety largely superfluous except in his more limited role of 'Gestellungsbürge'. Moreover, with the development of a more settled economy the normal creditor's main concern would be to have an assurance that, in the event of his debtor's bankruptcy or willful default, somebody else would be bound to

discharge the debt instead of him. Rather than a surety who would exercise pressure on the principal to meet his obligations, whether by compulsion or by pledging himself as a hostage, he wanted a surety who would, if necessary, pay the debt out of his own property. And this was precisely what the newer type of surety pledged himself to do.

In Germanic law the 'Zahlbürge' actually took over the legal position of 'debtor', and the principal was thus freed from all liability (*Haftung*) to the creditor,[12] though still bound, of course, to recoup the surety for all losses sustained by him in the event of his having to pay. It has been claimed that this represents the older rule, of which the 'accessory' liability of the Roman *fideiussor* was a modification. On the other hand, Irish law allowed the 'Zahlbürge' (*rāth*) to be sued only after all measures against the principal debtor had proved unavailing. But any attempt to establish an order of priority between these conflicting rules is, I think, based on a misconception. Since the 'Zahlbürge' is the product of a relatively advanced social and economic order, he is most unlikely to have emerged during the period of Indo-European unity, and each group must have called him into being independently. That he should have certain common characteristics in all of them is very natural, for he represents a development, and to some extent a continuation, of the two older types which were already in existence before the 'dispersal', the enforcing surety and the hostage-surety. This will, I hope, become clearer when we have examined the evidence provided by the Celtic lawbooks.

Most of this evidence comes from Ireland. The extant Irish legal sources are not only much older, in language and date, than those of Wales; they are also much more archaic in content. All lawyers tend to be antiquarians, and the Welsh jurists were no exception, but like the Romans, they used material from antiquity primarily in order to expound the genesis of the law of their own day. The Irish jurists, on the other hand, were incurably 'backward-looking':[13] they refused to discard a rule or institution even when it had been obsolete for centuries. Thus in the 'canonical' tracts which received their definitive shape during the seventh and eighth centuries, side by side with contemporary rules drafted in classical Old Irish we find passages which are more archaic alike in language and in content, and which doubtless go back to the oral teaching of the pre-Christian law schools. Further, Ireland, unlike Wales, had escaped the Roman discipline and its tribal institutions remained untouched by external influences until the coming of Christianity in the fifth century. An important by-product of evangelization was the introduction of writing in the Latin alphabet into a primitive society which in normal circumstances could not have developed a written tradition at so early a stage in its development. And when writing had seeped into the native schools from the monastic schools, the Irish jurists committed their traditional lore to parchment. Hence, thanks to a purely external stimulus, we have a written record of tribal institutions made at a stage of their development when similar communities elsewhere were still confined to oral transmission. For all these reasons

one might reasonably expect the Irish rules on suretyship, together with corresponding survivals in Welsh law, to shed considerable light on the earlier phases of this institution.

Modern knowledge of the subject is based almost entirely on Thurneysen's famous study, *Die Bürgschaft im irischen Recht.*[14] In this masterly work my great teacher collected virtually all the relevant material in the legal manuscripts, and for the first time explained the differences between the various types of surety—an astounding feat for one who (as he often lamented to me in private conversation) had had no training in legal history. His classification is basically right, and the present paper is designed merely to supplement it by showing how, even in the older sources quoted by him, one can already discern different stages of development. Among these sources an order of priority can and should be established. Thus in the long and difficult tract from which, as edited and translated by Thurneysen, most of our reliable information comes, we find relatively 'advanced' and 'primitive' rules formulated side by side. For the chief value of the Irish evidence lies in the fact that the jurists, in their unbounded reverence for the 'sacred' and allegedly 'immutable' law of the 'ancients', never jettison an archaic rule merely because it was no longer in force. They retain it alongside the rule that had superseded it, with the result that later commentators waste much time, ink, and parchment in vain attempts to reconcile the discrepancy, often in terms which suggest that in the practice of their time both rules were equally obsolete.

In classical Old Irish there were three words for surety, corresponding to the threefold classification I have outlined. All three have a common characteristic: they are abstract nouns, which, however, are also used in a concrete sense. (1) *naidm*, which normally means 'binding, bond' (cognate with Lat. *nexum*?) but which is often personified in the law tracts to stand for the 'binder', the person who is responsible for the bond, or as I call him here, the enforcing surety. (2) *aitire*, formed from the preposition *eter* (: Lat. *inter*, etc.), 'intermediacy', concrete 'the man between',[15] the hostage-surety. (3) *rāth* (fem. *ā*-stem), of uncertain etymology but probably connected with *rāith* (*i*-stem) 'rampart, fortification', the 'Zahlbürge', who by the time the canonical text of the tracts was stereotyped was already the normal type of surety.

There was, however, an older Common Celtic word for surety **makkos*, which gave Ir. *macc* and Welsh *mach*. And the history of this word in both dialects sheds some light on that of the institution also. In Irish, owing to inevitable confusion with the much more frequent homonym *macc* 'boy, son' (< **mak^{w}k^{w}os*, W. *map*), it became obsolete at a very early period, surviving only in a few archaic formulas, e.g. *aicc maccu* 'find (ipv.) sureties'. That the word which replaced it was *naidm* is proved beyond doubt by the substitution in several tracts of *naidm ⁊ ráth* for the older phrase *macc ⁊ ráth* which still survives in the archaic stratum of the texts. This of itself would tend to show that the earliest type of surety was the enforcing surety, whose function will be described presently. In Wales, on the other

hand, where there was no danger of confusion between *mach* and *map*,[16] the first word continued to be used for surety right through the Old and Middle Welsh periods. But the *mach* who meets us in the twelfth-century Welsh lawbooks has become to all intents and purposes a 'Zahlbürge', like the Irish *ráth*, while retaining (again like the *ráth*) some vestigial functions of the enforcing surety.

In the basic tract edited by Thurneysen the *ráth* holds the center of the stage, because by the time it was compiled he had largely ousted the two older types. Though both still survive, their original functions have been curtailed, at least in the contemporary sections of the tract. On the other hand, in the archaic stratum, consisting of aphorisms and obscure rhythmical passages which doubtless echo the oral tradition of the schools, we find the *naidm* (older *macc*) and the *aitire* figuring much more prominently. It is on these earlier sections of the tract (and on similar archaic passages cited in other tracts) that the following account of their original functions is based.

1. The three functions of a *naidm* are defined as 'holding in mind [the debt] for which he is invoked as surety, so that nothing be added to or subtracted from it, swearing to it without reservation, and enforcing it without negligence'.[17] The first two refer to an eventual dispute as to the existence or amount of the debt, when the *naidm's* word is decisive and must be confirmed by oath in case the matter comes to litigation. His third duty is the most important: he pledges himself to compel the debtor to perform his obligations. In pursuit of this objective he is invested with the widest powers: not merely can he use the normal method of enforcement, *pignoris capio*, by distraining the property of the debtor, he can also seize and imprison him.[18] And any violence he may use, even the slaying of a recalcitrant 'principal' is free from liability. Among the 'absolutely immune bloodsheddings' recognized by Irish law is 'blood spilt by a man who is enforcing his *naidm*' (here used in its primary abstract sense).[19]

Such drastic powers of execution suggest that this archaic type of surety belongs to the infancy of political organization and originated at a time when there was no judicial or executive machinery for enforcing private obligations. If so, the *naidm*, acting as a primitive substitute for judge and bailiff, must obviously have been a man of considerable standing in the tribal community. It may not be without significance that the later glossators invariably describe him as belonging to the 'grades of nobility' whereas the *ráth* or 'Zahlbürge' is always a commoner. Originally he may have been either the head of his 'principal's' kindred or (which is perhaps more probable) the lord to whom the average freeholder was attached in clientship. It is true that the main tract envisages every free man as capable of acting as *naidm*, but by the time of its compilation the *naidm* had been supplemented (though not, as we shall see, entirely replaced) by the introduction of a 'Zahlbürge'. Given the many privileges Irish law attached to aristocratic birth, it seems most unlikely that a mere commoner would ever have been invested with such far-reaching powers, and we may safely

conclude that, originally at least, the *naidm* must have been a member of the 'grades of nobility'.

The *naidm* pledged his 'honor' that the agreement guaranteed by him would be duly fulfilled. Hence failure by the 'principal' to carry out his obligations under it was an offense against the honor of his surety, one of the gravest offenses known to Irish law. The man who attempted to evade his commitments 'in breach of the honor of his *naidm*'[20] had to pay his surety the value of his 'honor-price' in addition to satisfying the original debt to the other party. Since this penalty, which was graded according to status, would usually exceed the amount of the debt—all the more so if (as I think likely) the *naidm* was originally drawn from the 'grades of nobility'—one can see that this additional mulct would be a further deterrent to the potential absconder. On the other hand, the *naidm* had no liability whatever toward the other contracting party for default by the principal; this was a vital difference between him and the other two types of surety.

Such, in brief, were the position and functions of the *naidm* in the archaic period. At least one German scholar has argued that the Roman *uindex* had originally the same position and functions,[21] though his view has not gone uncontroverted.[22] *Non nostrum . . . tantas componere lites.* I would merely point out here that were it not for the Irish jurists' congenital reluctance to discard obsolete rules, we would have a very different picture of the *naidm* as he meets us in the contemporary sections of the eighth-century tract, when the *rāth* or 'Zahlbürge' has become—like the Roman *fideiussor*—the normal type of surety. True, the *naidm* still survives; indeed he remains an indispensable part of any contract guaranteed by a 'Zahlbürge'. In every such contract there must be at least one *naidm* on each side, and all of them constitute, as it were, the overseers or protectors under whose auspices the contract is concluded. It is by them that the ritual formulas are addressed to the parties to the contract and their respective sureties (*rātha*). And when the debt falls due, the first attempt to enforce it is made by the *naidm* of the defaulting party, though whether he may still use such drastic means as imprisonment or bloodshed is uncertain. Only if his efforts are unavailing does the liability of the 'Zahlbürge' come into play. Accordingly he is still to some extent the guardian of the contract, and this residual function seems to confirm the theory that he was at one time the only enforcer as well as the only guarantor.

In the most solemn type of Welsh contract, the *amod*, each party is accompanied by what is called an *amodwr*, a 'contract-man', whose presence is necessary for the validity of the transaction. Both *amodwyr* are the official guardians of the contract; their evidence is conclusive as to its subject matter and amount, and should one of the parties default, the *amodwr* of the other party may enforce it by levying extracurial distress on his property.[23] But in the event of failure he does not become liable for the debt; indeed the other contracting party has no claim whatever on him. His function so closely resembles that of the *naidm* that I am inclined to

believe that the *amodwr*, too, is descended from the original enforcing surety.[24]

2. The *aitire* or hostage-surety, though by the eighth century clearly obsolescent, is still quite independent of the *naidm*, which reinforces the conjecture that the two types of surety are of separate provenance. Unlike the *rāth* (see *infra*), he is never associated with a *naidm*: in cases where he still acts as surety for purely private engagements within the tribe he pledges himself, not by a formal assurance to the *naidm*, but by an oath administered to him by the other party to the contract. Part of this runs: 'Swear by God that you shall be ready and willing to remain in stocks or in prison, with your foot in a fetter or your neck in a chain until you be freed therefrom by [payment by the principal of] the debts or until, having become forfeit, you give a pledge for yourself (i.e. to pay the normal ransom fee).' Hence the *aitire* guarantees, not with his property, but with his person only. Originally, perhaps, he had to surrender himself immediately on the conclusion of the contract, but by the time of our tract at latest his liability arose only when the appointed day for settlement had passed. Thereupon the creditor could, without proceeding against the defaulter, attach the person of the *aitire* by *manus iniectio*.[25]

It is most important to note that this surety may not free himself from liability, even after seizure, by discharging the debt out of his own property.[26] Instead, he must remain in custody for ten days, during which time he can be ransomed by his principal's paying the original debt (plus a surcharge for default). Should this happen, he is entitled to compensation for the 'disturbance' caused to him and to a refund of the expenses he has incurred during his captivity. On the other hand, if the ten days have elapsed without a settlement by the defaulting principal, the *aitire*, who has now become 'forfeit', may ransom himself by paying the normal fee, the full 'body-fine' of a free Irishman, a very formidable amount compared with the sum at issue between the contracting parties. In other words he must 'repurchase' the life and liberty he has technically 'forfeited'. This will involve the defaulting principal in crushingly heavy indemnities: besides paying twice the amount of the original debt to the creditor, he must reimburse the *aitire* for the 'body-fine' which was the price of his freedom and pay him his 'honor-price' as compensation for the indignity he has suffered. These subsidiary liabilities can now be levied by the surety himself.

The close resemblance between the *aitire* and the political hostage (*giall*) makes it clear that the first is merely an adaptation of the second to the domain of private obligations. And this is borne out by what was obviously the main function of the *aitire* at the time when the main tract on suretyship was compiled. Where two independent tribes have concluded (through their respective kings) a pact known as *cairde* (lit. 'friendship'), under which disputes between their members are governed by a kind of private international law, each of them nominates one or more *aitiri* as guarantors. If a member of one tribe subsequently defaults on his liabilities, whether

contractual or delictual, to a member of the other, the *aitire* of his tribe, should he fail to make him satisfy the claim, must surrender himself as a pledge to the creditor under substantially the same conditions as I have outlined above. It is surely significant that the institution should have lasted longest in this quasipublic capacity, thus underlining its original affinity to hostageship. As a matter of fact, in nonlegal literature both *aitire* and *giall* are used more or less indiscriminately for 'hostage', which suggests further that the *aitire* as surety for private engagements had been largely superseded by the 'Zahlbürge'.

At this point the Irish evidence may afford a clue to the way in which, to take but one example, the *egguētēs* of Greek law, who according to Partsch[27] and Lipsius[28] was originally a pure hostage, ultimately assumed the functions of a 'Zahlbürge'. In the archaic stratum of our tract the *aitire* who has become 'forfeit' must ransom himself like any ordinary captive by payment of the 'body-fine', and this discharges him from further liability under the contract. So much would seem to be clear from the concluding words of the oath sworn by him. But according to the eighth-century rule, the price of his freedom has been expanded to include, in addition to the normal ransom fee, the amount of the debt due from his principal. Thus a new type of *Haftung* has been created, and for the first time the surety becomes responsible for the actual obligation undertaken by the principal. A further step toward simplification would be to make the *aitire* redeemable *after* as well as before forfeiture by payment of the debt alone in place of the 'body-fine'. Eventually, with the establishment of a more stable polity, the archaic apparatus of 'hostageship' and 'forfeiture' would be discarded, and the surety's newer liability to pay the original debt—an arrangement which was certainly more agreeable to the creditor—would alone survive. In other words the 'Zahlbürge' has emerged from the hostage-surety. No doubt there were other factors in his evolution, and the various Indo-European peoples may not have fashioned him out of the same materials, but I think that the line of development suggested here accounts at least for the transition from *aitire* to *rāth* in Ireland.

3. By the eighth century the *rāth* has become the normal surety, and most of the tract edited by Thurneysen is concerned with him. Yet he has certain features which arouse the suspicion that he is a comparative latecomer. To begin with, he never seems to function independently, but always in association with (one might almost say in subordination to) the *naidm*, whose presence is essential, not merely to the conclusion of a validly secured contract, but also to the binding of the *rāth* himself. For among the formulas exchanged on this occasion is one in which the principal solemnly 'invokes' the *naidm* to 'guarantee' his promise to compensate the surety in case of default, and another in which the *rāth* 'invokes' the *naidm* to 'guarantee' his declaration that he will faithfully perform his duties as surety.

The chief of these duties is to satisfy his principal's obligations in the event of default. And the procedure is briefly as follows: When the debt

falls due, the *naidm*, who is now accompanied by the creditor, first attempts to enforce it against the debtor. Only if this attempt fails does the *rāth* become liable. In other words he cannot be sued before and in place of the principal, and here his legal situation differs from that of the Roman *fideiussor* (before Justinian introduced the *beneficium ordinis*), the Greek *egguētēs*,[29] and the Germanic 'Zahlbürge'.[30] On receipt of formal 'notice' from the creator, the *rāth* must give him a pledge for the amount of the debt plus one-third penalty, and this must be redeemed within a certain time either by the principal or by the *rāth* himself. If the principal redeems it by payment to the creditor, he must also compensate the surety for the 'disturbance' caused to him. On the other hand, if he defaults again and in consequence the debt is levied on the *rāth*'s property, his liabilities to the surety are indeed formidable: double the amount of the debt, the surety's honor-price and additional compensation for 'disturbance'. At this stage the *rāth* may act as his own 'enforcer' and distrain the property of the defaulter to the required amount.

Thus by the eighth century at latest Irish law had evolved a type of surety corresponding to the 'Zahlbürge' of the other Indo-European peoples. Was this an entirely independent evolution, or was it borrowed from outside? There is some slight evidence to suggest that the *rāth* emerged in the wake of Christianity,[31] but this is far from certain, and the transition from *aitire* to *rāth* may have taken place on the lines I have indicated without any external stimulus. What is certain is that the *rāth* shares, not only with the Welsh *mach*,[32] but also with the surety of the *Leges Barbarorum*[33] an absolute right to distrain a defaulting principal in order to secure payment of the debt and the compensation due to himself. In Wales and among the Germanic tribes, where the public administration of justice had made far greater progress than in Ireland, distress could normally be levied only after authorization by a court. Accordingly this residual right of extracurial distraint in those countries may be a survival from the days when the surety alone, without the assistance of the court or its officers, sought to enforce the private obligations he had guaranteed.

I suggest, then, that the Irish law of suretyship may well reflect the various stages in the development of this institution throughout the Indo-European world. Already during the period of unity two types of surety were known: the 'enforcer', who by reason of his superior status could compel the principal to discharge his obligations, thereby providing a primitive substitute for public sanctions, but who accepted no liability (*Haftung*) toward either of the contracting parties; and the 'hostage' (modeled on the political institution), a 'personal surety who staked his life and liberty that the principal would not default, and if he did so default became 'forfeit' to the creditor. The right enjoyed by the *aitire* to 'repurchase' his own freedom at the price of a 'body-fine' may well have been a secondary development. Still later payment of the original debt was substituted for the normal ransom, and the way was thus opened for the emergence of the 'Zahlbürge', who was pledged simply to satisfy the obligations of a de-

faulting principal out of his own pocket, but who (in the early period at least) retained some vestigial functions of the two older types of surety.

Notes

[1] *Quellen des röm. Rechts* (1953) 11.

[2] In the domain of law alone, e.g. oath, penalty, theft, judge, witness, etc.

[3] *Op. cit.* 41.

[4] It is hardly necessary to state that in describing conditions in primitive society words like 'contract', 'debt', 'creditor', etc., are somewhat anachronistic. At first suretyship was doubtless mainly employed in transactions between different kin groups, such as betrothals and 'treaties' for ending vendettas by means of wergild.

[5] *Lebor na Huidre*, Best and Bergin, eds. p. 295, *Revue celtique* xiii. 52, etc.

[6] Cf. *Ériu* xvii (1955) 67, 81.

[7] Cf. Ellis, *Welsh Tribal Law and Custom* ii. 3f.

[8] Cf. Liebermann, *Gesetze der Angelsachsen* ii. 332, Pollock and Maitland, *Hist. of Eng. Law*[2] ii. 191f.

[9] Some scholars see in him a substitute for the 'self-pledged' debtor who originally could act as his own surety by staking his liberty on the payment of his debt. But this form of 'Selbstbürgschaft' is much more likely to have developed from the system of hostage-suretyship; cf. Holdsworth, *Hist. of Eng. Law*[3] ii. 84. The 'debtor-surety' makes a very late appearance in both Irish and Welsh law.

[10] See O. W. Holmes, *The Common Law* 250.

[11] I am aware that most legal historians hold, on the contrary, that he assumed full liability for the debt. But the surety whose function they have investigated—in Greek, Roman or Germanic law—has already developed far beyond the hostage stage, and, apart from his special position in procedural engagements (bail, etc.), is a full 'Zahlbürge'. In the older stratum of Irish law, however, as we shall see presently, he is still a hostage and nothing more.

[12] See Schröder-von Künnsberg, *Lehrbuch der deutschen Rechtgeschichte*[6] 316.

[13] Frank O'Connor, *Kings, Lords and Commons* (1959) p. ix, has rightly noted this universal characteristic of the Irish literary class; it applies with special force to the jurists.

[14] *Abhandlungen d. preuss. Ak. d. Wissensch.*, 1928, *phil.-hist. Klasse nr. 2*; addenda and corrigenda in *Zeitschr. f. celtische Philologie* xviii (1930) 364–408. In the present paper the main work is referred to as '*Bürgsch*'.

[15] It is probably just a coincidence that the Sanskrit term for surety *pratibhu* is interpreted as *madhyastha* 'medio stans', 'middleman' by a commentator on Panini; cf. Partsch, *Griechisches Bürgschaftsrecht* 37 n. 4.

[16] Pedersen, *Vergl. Gramm. d. kelt. Sprachen* i. 128, held that Ir. *mac(c)* 'surety' was the same word as *mac(c)* < *mak^{w}k^{w}os* and that W. *mach* was simply a borrowing of the Ir. word used in this specialized sense. One might conjecture that since men so often gave their sons as hostages, and since suretyship was in origin simply an adaptation of hostageship to matters of private law, the Irish at first used the same word for both and were copied by the Welsh. But the arguments against this theory are overwhelming. (1) Among the comparatively few words borrowed into Brythonic from Goidelic there is no other example of a legal term. (2) The name of an ancient and well-established institution is most unlikely to have been borrowed at a relatively late stage of linguistic development. (3) Neither Ir. *macc* nor W. *mach* means a hostage-surety. Hence Thurneysen (*Bürgsch.* p. 4) is surely right in rejecting Pedersen's view (which, incidentally, is not repeated in the shortened version of his Grammar in English).

[17] *Bürgsch.* 13 §43.

[18] This is implied by the opposite rule which imposes a stiff penalty for 'shackling a man who has not invoked thee as *naidm*', *Anc. Laws of Irel.* v. 476 (cited *Bürgsch.* 57).

[19] *Ibid.* 142, 9.

[20] Ir. *tar enech nadma*, 'lit. across the face of his *naidm*', the same word being used in Ireland, as so often elsewhere, for 'face' and 'honor'. Significantly enough, this phrase is never used of the *aitire* or the *rāth*.

[21] Leifer, Zum römischen *vindex*-Problem, *Zeitschr. f. vergl. Rechtswissensch.* 50 (1935) 5–62.

[22] Koser, *Das altröm.* ius (1949) 194f., etc.

[23] Walter, *Das alte Wales* 430; Ellis, *op. cit.* ii. 5f., 16.

[24] Walter (*loc. cit.*) compares him to the *adstipulator* in older Roman law (cf. *Gaii Inst.* iii. 110 ff.), about whose original functions little is known for certain. Was he also, perhaps, a relic of an archaic enforcing surety?

[25] In Pauly-Wissowa, *RE* vii A, 2054ff. A. Steinwenter has arrived at a somewhat similar conclusion about the position of the Roman *uas*, whom, however, he takes to have been solely a 'Gestellungsbürge'. But may not this specialized function be a later restriction?

[26] This seems to afford striking confirmation of Partsch's 'hypothesis' (*op. cit.* 76) that the 'original *égguos*' could not free himself by performing the obligation on which his principal had defaulted.

[27] *Op. cit.* 14, 24f., 40f.

[28] *Das attische Recht u. Rechtsverfahren* 706.

[29] *Ibid.* 712.

[30] Schröder-von Künssberg, *op. cit.* 316.

[31] A 'canon' ascribed to St. Patrick, but almost certainly not earlier than the sixth century, provides that if a cleric becomes surety (*fideiusor*) for a layman (*pro gentili homini*, less probably 'for a pagan') and 'per astutiam aliquam gentilis ille clerico fallat, rebus suis clericus ille soluat debitum. Nam *si armis conpugnauerit cum illo*, merito extra ecclesiam conputetur' (ed. Bieler, *The Irish Penitentials* (1963) 54; italics mine). In other words the cleric is forbidden to act as a *naidm* since this would involve the use of violence; instead, he must pay the debt out of his own property if the principal defaults. A possible inference from this is that at the time when the canon was issued the *naidm* was the usual type of surety, so that the idea of a 'Zahlbürge', modeled on the Roman *fideiussor*, was introduced by the Church. But it is on the whole more likely that the *rāth* was already in existence.

[32] Cf. Ellis, *op. cit.* ii. 246ff.

[33] e.g. *Lex Burgundionum* xix. 5, 6; Schröder-von Künnsberg, *op. cit.* 316f.

Mythological Reflections of Indo-European Medicine

Jaan Puhvel

In *Yašt* 3.6 (to Aša), Old Iranian medical specialists are listed as the one who heals by *aša* (*ašō.baēšaza-*), the one who heals by 'the law' (*dātō. baēšaza-*), the one who heals by the knife (*karətō.baēšaza-*), the one who heals with herbs (*urvarō.baēšaza-*), and the one who heals with spells (*mąthrō.baēšaza-*). In *Vidēvdāt* 7.44 the list is limited to the last three. In both instances the last type is called 'the healingest of the healing' (*baēšazanąm baēšazyō.təma-*). As early as 1876 this shorter version attracted the attention of James Darmesteter[1] in relation to Pindar's Third Pythian Ode (40–55), where Asklepios' healing art is applied to helping those who come to him afflicted with spontaneous sores, or wounded by metal weapons or stone-throws, or ravaged by seasonal fevers; he treats some with soothing incantations (*malakaîs epaoidaîs*), others with healing potions (*prosanéa pínontas*), still others with external medicaments (*guíois peráptōn pántothen phármaka*), and sets some on their feet with incisions (*tomaîs*). Darmesteter concluded: 'Voilà, dans ses grandes divisions, le Manuel de l'étudiant en médecine dans la période de l'unité indo-européenne.'

This striking parallelism was subsequently duly codified in various handbooks[2] but received no incisive attention until 1945, when Émile Benveniste's article 'La doctrine médicale des Indo-Européens'[3] revived the comparison. In the meantime A. Meillet had emphasized the importance of independent tertia comparationis in arriving at Indo-European reconstructions, and G. Dumézil had come to stress the role of ideological structures in Indo-European social and mythical thinking. Benveniste's paper[4] reflects both these lessons. A Vedic comparison is adduced (*RV* 10.39.3) where the Aśvins are said to be the healers of the blind, the emaciated, and the fractured (*andhásya cin nāsatyā kṛśásya cid yuvám íd āhur bhiṣájā rutásya cit*). The list is taken to present the afflictions to which the spell, herb, and surgical cures respectively apply, and thus to confirm not only an Indo-European 'doctrine' but its tripartite basis in social structure (priests-spells-blindness, warriors-knives-wounds, farmers-herbs-weakness). By way of preamble, the use of IE **med-* in a medical sense in Avestan (*vī-mad-*) and Latin, as opposed to diverse wider meanings in the rest of Indo-European (Oscan *meddíss* 'judge', OIr. *midithir* 'judge', Gk.

médomai 'be concerned', *mḗdomai* 'decide', Goth. *mitan* 'measure,' Arm. *mit* 'thought'), is taken to indicate the rational, authoritative nature of Indo-European medical measures (cf. Lat. *modus*, *moderor*, *modestus*).

Benveniste's data belong in the considerable and impressive amount of Indo-European formulaic trifunctional débris which he and Dumézil have successfully collected,[5] and are included in Dumézil's summation of such material in *L'idéologie tripartie des Indo-Européens* 7–32 (Brussels, 1958).[6] The tertium of Benveniste's comparison is, however, open to criticism. He is juxtaposing a rather late Vedic (*RV* maṇḍala 10) passage with a late Avestan one (*Vidēvdāt*) and with a fifth century B.C. Pindaric ode.[7] All of the attestations occur in an area which was contiguous in the Indo-European orbit and where manifold and demonstrable cultural cross-currents prevailed at least from the Achaemenian ascendancy onward (sixth century B.C.). We may leave aside the wider ranges of intellectual influences[8] and concentrate on the natural and medical sciences. The profound similarities between the physiological wind-doctrines of the early Ayurvedic *Caraka*, *Bhela*, and *Suśruta Saṁhitās*, and the Hippocratic pneumatism of *Perì phusôn*, *Perì hierês noúsou*, and *Perì phúsios anthrṓpou* are a matter of record. In pathology, the non-Hippocratic Platonic views on the nosopoeic trinity *pneûma* 'breath', *phlégma* 'phlegm', and *kholḗ* 'bile' (*Timaeus* 84d–86a) are closely matched by the classical Ayurvedic teachings concerning the synonymous *vā́ta-*, *śleṣmán-*, and *pittá-*.[9] Plato's theory on the nature of sight as based on an intraocular form of fire (*Timaeus* 45b–d) is paralleled by Suśruta (*Sūtrasthāna* 21.7), and similar accordances may be multiplied. Because they antedate the forays of Alexander into India, the contacts must have an Iranian intermediary. It is in fact well known that Greek physicians were in attendance at the Achaemenian court (e.g. Demokedes, Apollonides of Kos, Ktesias of Knidos), that they enjoyed a high reputation, and had access to information concerning India (as proved by Ktesias' writings, preserved in summaries by Photios). Even though a reactionary ode-maker was less likely than Plato to purvey 'Persische Weisheit in griechischem Gewande',[10] it is not inconceivable that Pindar's Asklepios-passage may reflect a borrowed Iranian or Indic-Iranian concept of the healing arts. A Hippocratean aphorism (7.87) states that: *Hokósa phármaka ouk iêtai, sídēros iêtai. hósa sídēros ouk iêtai, pûr iêtai. hósa dè pûr ouk iêtai, taûta khrḕ nomízein aníata.* It seems to me that we have here in ascending order the same trifunctional cures as in Pindar or the *Vidēvdāt*, except that the emblem of the fire-cult of the Magi as the most prestigious form of cure has taken precedence over verbal ministrations. Much later, in the *Dēnkart* (Book 3), spells, fire, herbs, knife, and 'burning' (fumigation or cauterization?) are distinguished, which merely attests to an encyclopedic assembling of the split formulas. A. Christensen[11] sees Greek influence everywhere in Sassanian medicine, which for that time may well be true; yet the Iranian source of the Hippocratic formula a millennium earlier is not implausible.

It may thus be doubted that the Greek, Iranian, and Indic formulas

adduced by Benveniste are three independent building blocks for an Indo-European reconstruction. Otherwise one must assume a fossilized Indo-European tradition in the middle of a Pindaric ode, comparable to the scraps of trifunctional lore which have been dredged up in Greece.[12] One could maintain that if the pre-Kleisthenic Ionian tribal groups and similar divisions (see *KZ* 79.6 [1964]), the Iranian social classes, and the Indic castes may all be independently descended from an Indo-European prototype, why not types of healing? The objection lies simply in the excessive degree of similarity and the demonstrated fact of international interpenetrations of medical lore by the fifth century B.C.

While the reconstruction of a prototype via the traditional comparative method is thus not unchallengeable in our particular instance, we should not discard the possibility that the notions involved are Indo-European. We must merely deepen and diversify our approach. For our purposes Indo-European medicine is a social, religious, and mythical phenomenon, not a scientific or historical one. Rather than pursuing only select formulaic analogies and constructs, we should discover as fully as possible the relations of medico-mythic data to the social and cultic structures of which they formed a part. When such systematic materials are established for the Indo-Iranian (and Greek) area, a firm ground exists for some measure of comparativistic confrontation with the Indo-European West. There is no reason to assume that those Indo-European subgroups which yield little or nothing for Indo-European comparative mythology (Anatolian, Tocharian) would be any more rewarding for the study of Indo-European mythical medicine.

It is a well-known, almost a banal feature that hymnic treatments of gods tend to agglomerate and universalize the powers and functions of the object of celebration. Thus the Aśvins or Asklepios in the passages quoted are represented as the all-round practitioners of healing, whereas the earth- and fee-bound Avestan physicians remain compartmentalized in their specialties. It is methodologically sound to postulate that early Indo-European medicine was likewise split up among the representatives of the three social classes, and that the priests, warriors, and agriculturalists had their specialties of verbal, surgical, and pharmaceutical cures. It is advisable to assume that the gods, the supernatural projections of the same social divisions, were similarly structured in their healing aspects. We should thus expect a separate and specific healing presence in each component of the tripartite pantheon.

Such is indeed the case in Vedic India. In contrast to the stray involvement of various other divinities, there are the towering healing deities Varuṇa, Rudra, and the Aśvins.

Varuṇa, the upholder of *ṛtá-*, is ipso facto the guardian against all sinful deviation from this cosmic norm, including disease, which is the result of such sin. In reprisal he 'binds' the guilty, i.e. afflicts him with crippling illness. The nature of the 'bonds' is not made clear in the Rigveda, but from the Atharvaveda onward it is most especially *jalodararoga-*, i.e.

'water-belly' or dropsy; cf. e.g. *AV* 4.16.7: *śaténa pā́śair ábhi dhehi varuṇainaṁ mā́ te mocyanṛtavā́ñ nṛcakṣaḥ/ástāṁ jā́lma udáram sraṁsayitvā́ kóśa ivābandhráḥ parikṛtyámānaḥ* 'with a hundred fetters confine him, Varuṇa, let not the untruth-speaker escape you, watcher of men; let the culprit sit with his belly falling out, like an unhooped vessel cut round about'. Something of the later 'Varuṇa und die Wasser' connection is already present in the Rigveda and doubtless in the watery nature of Varuṇa's bestowal. Yet more importantly, Varuṇa figures as the magical sovereign who sends disease but can also lift it. What matters is the purely sacral concept of the cause of affliction and its removal solely in terms of prayers and invocations. These procedures were the monopoly of the Brahmans, as is most clearly brought out in the story of Śunaḥśepa (*Aitareya-Brāhmaṇa* 7.13–16): Childless King Hariścandra vowed to sacrifice to Varuṇa any son he might have, but when Rohita was born he temporized until finally Varuṇa pressured him into compliance. Rohita, now grown up, escaped to the forest, while Varuṇa seized the king, and his belly swelled up. After years of wandering Rohita at length found a starving Brahman who sold him one of his three sons, Śunaḥśepa, as a substitute victim. When Rohita offered to redeem himself and his father with this victim, Varuṇa consented, because *bhūyān vai brāhmaṇaḥ kṣatriyād iti* 'a Brahman is better than a Kṣatriya'. Śunaḥśepa was bound to the stake at the *rājasū́ya-*, but started to recite prayers to various deities in succession. As each verse was said by him, 'a bond was loosened, the belly of Hariścandra became smaller'; when the last verse was said, the last bond was removed and the king became free from disease. A typical formula of this priestly Varuṇa-medicine is seen in *RV* 1.24.9: *śatáṁ te rājan bhiṣájaḥ sahasrám urvī́ gabhīrā́ sumatíṣ ṭe astu/bā́dhasva dūré nírṛtiṁ parācaíḥ kṛtám cid énaḥ prá mumugdhy asmát* 'a hundred cures are yours, a thousand, King; wide, deep be your favor; banish disintegration far, far away; remove from us any sin we have committed'.

If Varuṇa's name is a personified abstraction,[13] that of Rudra is purely epithetal (*ródas-* 'heaven': *rudrá-* 'heavenly', like e.g. *ójas-*: *ugrá-*),[14] like his allonyms *Śarvá-* 'Destructive' and *Śivá-* 'Propitious'. In spite of the problems of post-Rigvedic expansion and non-Aryan elements in the constitution of this deity, his close Rigvedic ties to Indra and his sons the Rudriyas-Maruts, and the typological character of his name (in the trifunctional Rudras, Vasus, and Adityas of *RV* 10.125.1) vouchsafe his kṣatriya-nature and the antiquity of his origins. The Indo-Iranian background of Rudra is indicated by the Avestan Saurva who has joined Indra and Nā̊ŋhaithya in the Zoroastrian pandemonium (*Vidēvdāt* 10.9, 19.43).

Rudra is the archer who inflicts disease, thus the Vedic exponent of a world-wide nosological concept.[15] The manifestations of Rudra's onslaught are not like the mysterious internal symptoms of Varuṇa's dropsy but rather fever (*takmā́*, *AV* 11.2.22), cough (*kā́sikā*, *ibid.*), and 'poisons' (*viṣáṇi*, *AV* 6.90.2). Like Varuṇa he is ambivalent in his role (bowman and healer: *RV* 5.42.11); the well-known concept *ho trósas iásetai* is expressed

by *AV* 2.9.5: *yáś cakā́ra sá níṣkarat sá eva súbhiṣaktamaḥ/sá evá túbhyam bheṣajā́ni kṛṇávad bhiṣájā śúciḥ* 'he that did it shall undo, he the best of healers; he shall make cures for you through the physician, he the clean'. Rudra is copiously referred to in this laudatory vein, e.g. *RV* 2.33.4: *ún no vīrā́ṅ arpaya bheṣajébhir/bhiṣáktamaṁ tvā bhiṣájaṁ śṛṇomi* 'cause manly men to grow through your ministrations; you are reputed the best of physicians'. Like Varuṇa, he has 'a thousand cures' (*RV* 7.46.3). In *RV* 2.33.7 Rudra's soothing hand is said to heal and dispel divinely inflicted disease generally (*apabhartā́ rápaso daívyasya*), intimating that his healing functions may transcend the reparation of his own attacks. All told, Rudra's healing aspects are most strongly developed in the earlier Vedic texts, though he remains the 'first divine physician' (*prathamó daívyo bhiṣák* [*YV TS* 4.5.1.2, *YV VS* 16.5]) throughout the Vedic period. Subsequently the vast development and elaboration of the Śiva-figure of classical Hinduism makes him preeminently a destroyer and submerges the beneficent traits of his character.

In connection with Rudra certain Greek data inject a complex and disturbing comparativistic note. Using H. Grégoire's interpretation of Asklepios as the mole-connected (*skálops*, *aspálax*) healer-hypostasis of the plague-shooting Apollo Smintheus (*sminthos* 'rat'), R. Goossens appended the parallel ties of Rudra with *ākhú-* 'mole, rat'.[16] To summarize: Apollo is the plague-dealing archer (*Loímios*) who shoots beasts and men (*Iliad* 1.50), but he is also a healer (*Paiéōn*) of men and cattle. His son Asklepios is specialized as healer alone; the forgotten mole-connection of Asklepios' name reveals an ancient symbolism of the mole as the blind, innocuous, beneficent, chthonian permutation of the Apollinian plague-rat. Apollo is also known as *Loxías* (cf. *loxós* 'slanting, crosswise, oblique') and is god of incantations and poetry. For Rudra's Vedic role, cf. *RV* 1.114.10 *āré te goghnám utá pūruṣaghnám* 'away with your killings of cattle and men'; but he likewise heals beasts and humans (*RV* 1.43.6, 1.114.1). Rudra has the mole as his animal (rather than rat, as contexts show); cf. *YV VS* 3.57 *Rudra . . . ākhús te páśuḥ*, *MS* 1.10.4 *ākhúṁ te Rudra páśuṁ karomi*, *KS* 9.7, 36.14 *Rúdra ākhúṁ te páśuṁ karomi*; further *TS* 1.8.6.1, 6.6.45, *TB* 1.6.10.2, *ŚB* 2.6.2.10, *Āpast. Śrautasūtra* 5.9.8, 8.17.9 and 11, 14.7.3. He is also known as *Vañkú-* in *RV* 1.114.4 (cf. *vañc-* 'go crookedly, totter'). His son Gaṇeśa, who is his hypostasis with an epithetal name (= *gaṇapati-*), is the patron of poetry and is associated with *ākhú-* in the sense of 'rat' (also with the elephant, etc.).[17] Rudra's medicinal role disappeared in later times, nor did Gaṇeśa inherit it, nor did the mole-tradition survive, only the rat. In Greece the medicinal function of Apollo survived and was emphasized in Asklepios; yet here also the mole-figure was obscured, whereas Apollo Smintheus was still worshipped in Strabo's time as *bebēkòs epì tôi muí* (Strabo 13.48; cf. Gaṇeśa's epithet *ākhu-ga-*).

Obviously a good deal of zoomedical popular symbolism has gone into or been grafted onto these traditions; in more than one sense we deal here with a chthonian folkloristic underground. The principal question arises

to what extent one should dare approach profoundly localized Greek traditions and impute to them underlying Indo-European mythic inheritances overlaid with new material. Not since Leopold von Schroeder compared Apollo with Agni (*KZ* 29.193–229 [1888]) had anyone seriously broached this game. The Thessalian healing daimon Asklepios of Trikka, born of the Dionysiac-type female Koronis and later ensconced in the molehill-structured tholos of Epidaurus, would seem eminently impervious to Indo-European elucidation. Yet it is undeniable that the Dioscuri, for instance, distantly mirror the Indo-European Divine Twins despite all their Laconian particularism. In the same manner the possible connections of Apollo-Asklepios with an Indo-European mythologem cannot be dismissed outright, especially in conjunction with the probable linguistic equation *Paōn*:*Pūṣán-* (like *Aṓs*:*Uṣā́s-*; W. Schulze, *Kleine Schriften* 217–18), the cattle-related, itinerary, and psychopompous similarities between Pūṣan and Pan's father Hermes, and the name-identity of Asklepios' grandfather Phlegyas with the warrior-function king of Orchomenos (see F. Vian in footnote 12).

Varuṇa had cures but is not himself referred to specifically as physician. Rudra is called *bhiṣák* many times, but the details of his ministrations are unstated. The Aśvins, on the other hand, were fully versed general practitioners, as advertised in *RV* 10.39.3 (above), dealing in ophthalmology, strengthening cures, and bonesetting alike. These popular, uniformly beneficient tutelary divinities of the third estate became the ideal repositories of the entire range of curing activity in exact proportion to the progressive demythization of healing, as attention shifted from supernatural causes to earthly cures. In addition, as the Aśvins make the blind and lame see and walk (*RV* 1.112.8 *prāńdhám śroṇáṁ cákṣase étave kṛtháḥ*), or restore Cyavāna's sexual vigor in a rejuvenation stunt (*RV* 1.116.10), they are acting out a folkloristic role of rescue and miracle-working rather than a strictly medical performance; hence the emphasis is on the symptoms and results rather than the means and procedures of healing. It thus seems that the healing function of the Aśvins is mainly just another aspect of their more general philanthropic activity of saving from distress,[18] although they are called *daívyā bhiṣájā* in *RV* 8.18.8 and later (*devā́nām bhiṣájā* in *YV VS* 27.9; cf. *MS* 4.6.2, *KS* 27.4), all the way down to Caraka (*aśvinau devabhiṣajau* in *Cikitsāsthāna* 1.4.40–47, where a short course on mythomedical lore is given). In Ayurvedic tradition the Aśvins become the standard divine source of human medical knowledge. Granted their sublimated Iranian counterparts Haurvatāt ('Wholeness, Health') and Amərətāt ('Long Life'), and the connection of these with water and plants respectively, one should, however, postulate a basic original relationship of the Aśvins to herbal cures (cf. the *Oṣadhīstutiḥ RV* 10.97) and to the healing function of waters (e.g. *AV* 6.91.3: *ā́po víśvasya bheṣajī́r tā́s te kṛṇvantu bheṣajám* 'the waters are remedial of everything, let them make remedy for you').[19] One suspects that the disease or diseases called *yákṣma-* in Vedic tradition, and matched by the Ayurvedic *yakṣman-*, which probably

have a consumptive, phthisic character, were particularly suited to treatments by herbs and potions (as in *RV* 10.97.11–13).[20]

The vaiśya-environment, which was patronized by the Aśvins and constituted the principal repository of ethnomedical folklore, was the scene of verbal healing practices quite different in kind from 'Varuṇaic' invocations. Popular magic is quite commonly a 'third function' matter rather than referrable to the sovereign high gods (witness the *seiðr* of the Old Norse Vanir). An obviously Indo-Iranian example of such practice is the hymn for the expulsion of *yákṣma-* from all parts of the body (*RV* 10.163, *AV* 2.33), which incidentally supplies a valuable anatomical vocabulary list. Its close similarities to *Vidēvdāt* 8.35–72 were exposited by J. Hertel in an appendix (377–85) to R. F. G. Müller's 'Die Medizin im Ṛg-Veda' (*Asia Major* 6.315–87 [1930]), which latter is itself rendered unenjoyable by the omnipresence of Hertel's 'Arische Feuerlehre'. The subject of the Avestan passage is a series of piece-by-piece ablutions of the body as prophylaxis against disease and destruction wrought by the incursion of the druǰ Nasu upon contamination from corpses. Although it is formulated as a dialogue between Zarathuštra and Ahura Mazdāh and makes use of the *gaomaēza*-rite (see below), it is basically an ethnomedical incantation of a popular kind. If such matter passed under the name of *mąthrō.baēšaza-*, we may assume that Iranian herb and spell medicines were both in origin third-estate practices, and that consequently the *mąthrō.baēšaza-* of the later Avesta is not the Iranian equivalent of the Brahmanic Varuṇa-medicine of the Rigveda; it must have originated among the *vāstryō.fšuyant-* rather than the *āthravan-*. Its pride of place in the Avestan scheme of healing (*baēšazanąm baēšazyō.təma-*) indicates an intrusion of popular magic into latter-day priestly practice.

One is then entitled to wonder whether the *ašō.baēšaza-* and the *dātō.baēšaza-* of the *Aša Yašt* are in fact gratuitous accretions, or whether we do not here have a petrified canonical list of Indo-Iranian mythic medicine in the true tripartite pattern:

ašō.baēšaza-: cf. Ved. *ṛtá-*, the property of Varuṇa.
dātō.baēšaza-: cf. Ved. *dhā́man-*, the law of Mitra-Varuṇa.
karətō.baēšaza-: cf. Xšathra, connected with metal.
urvarō.baēšaza-: cf. Amərətāt, connected with plants.
mąthrō.baēšaza-: medical spells of magic character.

For the practical purposes of the *Vidēvdāt* the first two types were obsolete, nor does the Varuṇaic prayer-healing of the Rigveda show any tendency to absorb other types of medicine. In the other two classes, however, syncretism is rife: not only are the Aśvins all-round healers, but the *Oṣadhīstutiḥ* is to release from Varuṇa's and Yama's fetters (*RV* 10.97.16), and Soma, the King of Herbs (ibid. 18–19), joins Rudra in a devatā-dvandva (*RV* 6.74) to dispel sickness, drive out Nirṛti, and free victims from Varuṇa's bond. These are simple syncretistic encroachments and do not exposit any rivalries between types of healers. Such opposition was once suspected by K. F. Geldner[21] between Rudra and the Aśvins,

although exegetic refinements of most of the relevant details now render his interpretations obsolete.[22] That there were clashes between schools of medical practitioners is a priori probable, and one should be attuned to possible mythical reflections of them.

We have discussed data concerning Indic (and some Greek) gods and about Iranian physicians but have not so far touched on Iranian gods as direct evidence. The mythological content of the Gāthās, in addition to being abstracted, makes no reference to medicine. Our only hope lies in post-Gāthic deities who weathered the reform and reappeared in later Iranian tradition. No doubt in narrow Zarathuštrianism the healing functions were partly assumed by the Amәša Spәntas, partly lost in such proscribed daēvas as Saurva and Nåŋhaithya.

At the very end of the preserved Avesta (*Vidēvdāt* 22) is a mythical explanation of the long decontamination rituals which precede it, notably the potent *gaomaēza-* (within rectangular precincts formed of ternary groups [3, 6, 9] of ritually drawn furrows the victims of pollution bathed in holes filled with bull urine and then with water): At the dawn of creation Aŋra Mainyu created 99,999 illnesses for Ahura Mazdāh. The latter turned to Mąthra Spәnta ('Potent Spell') and offered a thousand swift steeds as recompense for healing, but Mąthra did not feel up to the task. Then Ahura Mazdāh sent Nairyō.saŋha (= *RV Nárāśáṁsa-*, a form of Agni) to the house of Airyaman with the same request. Airyaman promptly came to the mountain and to the tree which was the scene of the sacred conversations of Ahura Mazdāh and Zarathuštra. He brought hairs from nine stallions, nine male camels, nine steers, and nine rams; he also had nine sticks and drew nine furrows (thus establishing the *gaomaēza-*).

Airyaman also figures importantly in the prayer *ā airyәmā išyō* (*Yasna* 27.5, 54.1; *Vid.* 20.11) which probably formed part of the marriage ritual. The generous Vedic Aryaman is preeminently connected with the institution of marriage, and although not explicitly a healer, is occasionally invoked (along with Pūṣan) in maieutic context (*AV* 1.11.1). Dumézil (*Le troisième souverain* [Paris, 1949]) has clarified Aryaman's nature as one of the minor Adityas, especially close to the god of contracts, Mitra (similarly Airyaman in the marriage prayer is invoked most closely with Vohu Manah, the Amәša Spәnta corresponding to Mitra). Aryaman-Airyaman is a first-function figure whose eminent domain of concern strongly involved the third function. The important difference between Aryaman and third-function deities is that his actions are mythically founded and precedent-setting in a social context, whereas the succor tendered by the Aśvins is individually tailored and for the nonce. The Aśvins revive a tottering marriage (e.g. that of Cyavāna): Aryaman patronizes all marriages. The Aśvins heal whatever happens to the ailing: Airyaman disposes of all maladies in one primordial mythic enactment.

In confrontation with Airyaman there is all the more reason to believe that the bulk of what constitutes the *mąthrō.baēšaza-* is not in origin a first-function form of cure but rather a third-function one based on popular

rites. The shunting aside of Mąthra Spənta in favor of Airyaman in the etiology of the *gaomaēza-* indicates incompatibility in mythical terms. The grotesque nature of the ritual should not faze us into postulating a barnyard origin: we are rather dealing with the potent excretions of what distantly reflects the primordial Bull (or Cow, to use the etymon) of Mazdean cosmogony and the rituals of Mithra whose close satellite Airyaman must once have been in Indo-Iranian times.

In the *Aša Yašt* (3.5), just before the listing of *ašō.baēšaza-*, etc., 'Airyaman' (i.e. his prayer?) is said to be the greatest of *mąthra*'s. This passage shows that the Airyaman relics of original first-function healing were still considered inherently superior to the run-of-the-mill *mąthra*'s of popular origin which came to constitute the bulk of the *mąthrō.baēšaza-*.

Thus there seems to be evidence for a Mitraic-Aryamanic medicine beside the Varuṇaic one, centered on the foundation of healing rituals as opposed to the appeasement of the dread Varuṇa. Mitra's Vedic pallor has hidden its Indo-Aryan manifestations, and Mithra's Iranian transformation has almost wiped out Iranian attestations, except for the Airyaman passages. We are also in possession of a Celtic confirmation which has been discovered by Dumézil (*Le troisième souverain* 167–86).

Airyaman's cathartic role was not confined to the etiology of the *gaomaēza-*. He also figures eschatologically in the *Bundahišn* (30.19–20)[23] as the agent who along with fire melts the metals into the purifying bath leading to Fraškart. In Vedic tradition (esp. Sāyaṇa ad *RV* 1.139.7), Aryaman in the dawn of time helped the Angirases milk the Cow Kāmadhuk, thereby assuring the success of ritual acts for evermore.[24] The Angirases are closely connected with the *pitáraḥ*, the ancestors dwelling in the Vedic Otherworld whose chieftain Aryaman becomes in Epic and Classical tradition. His connection with roads and paths is found already in Rigvedic tradition and becomes notorious thereafter, with a shift from the hymenaic to the psychopompous, much as in the case of Pūṣan. Somehow this extension of Aryaman's nature implies a supersession of Yama and Varuṇa, those boon companions of the Otherworld (*RV* 10.14.7) from whose disease-bonds release is craved in *RV* 10.97.16.

The Irish Eremon, likely enough etymological cognate of Aryaman (IE **Aryo-men-*), is the king of the Sons of Míl, i.e. the leader of the last invasionary contingent of the mythological cycle, the Celtic inhabitants who succeed the Indo-European divine settlers, the Tuatha Dé Danann previously ensconced on Hibernian soil, and put them in their supernatural places in the mounds. In Indo-European mythical terms the 'third sovereign' Eremon joins the Mitra-Varuṇa pair of Nuadu and Lug as the specific champion of the *aire* (plur. *airig*). We thus have here a first-function figure also connected with the third estate (the *bó airig*) and euhemerized into a legendary chieftain. Eremon built causeways and royal roads.[25] According to passages in the *Book of Lecan* and the *Book of Leinster* he also provided wives to the Cruithnig (Picts). Most importantly, in order to save Eremon's ally Crimthann's army from destruction by poisoned

darts at the hands of a Breton enemy, a druid of the Cruithnig devised a cure which consisted in pouring the milk of six-score white cows into furfows on the battlefield. Eremon is thus connected with roads, marriage, and collective cures involving bovine liquids. In summation, the Indo-European **Aryo-men-* may be postulated as a sovereign-function hypostasis of the Mitra-type, a deified abstraction meaning 'Aryanhood' in the sense of socioreligious compacts and establishments relating to the operation of communal welfare and health.[26]

Another tradition of the mythological cycle contains important comparative material for Indo-European medicine. After Nuadu's arm was cut off in the First Battle of Mag Tuired (a Western Indo-European 'Mitraic' mutilation corresponding to that of the Norse Týr and the Roman Scaevola), Bress son of Elada took the kingship of Ireland, until the arm of Nuadu was 'healed' by the physician Dian Cecht: 'A silver arm with full activity in every finger and every joint did Dian Cecht set upon him, Credne the wright helping him. Miach son of Dian Cecht set joint to joint and vein to vein of his own hand upon him, and in thrice nine days was it healed . . .'[27] In the account of the Second Battle of Mag Tuired[28] there are further details and data: §33: 'Now Nuadu was in his sickness, and Dian Cecht put on him a hand of silver with the motion of every hand therein. That seemed evil to his son Miach. He went to the hand, and he said 'joint to joint of it and sinew to sinew', and he healed Nuadu in thrice three days and nights . . .' §34: 'That cure seemed evil to Dian Cecht. He flung a sword on the crown of his son's head and cut the skin down to the flesh. The lad healed it by means of his skill. Dian Cecht smote him again and cut the flesh till he reached the bone. The lad healed it by the same means. He struck him the third blow and came to the membrane of his brain. The lad healed it also by the same means. Then he struck the fourth blow and cut out the brain, so that Miach died, and Dian Cecht said that the leech himself could not heal him of that blow.' §35: 'Thereafter Miach was buried by Dian Cecht, and herbs three hundred and sixty five, according to the number of his joints and sinews, grew through the grave . . .' Later Dian Cecht states as a matter of general medical fact: §99: 'Every man who shall be wounded there, unless his head be cut off, or the membrane of his brain or his spinal marrow be severed, I will make quite whole in the battle on the morrow.' In §123 the healing procedure of Dian Cecht is described (and, inconsistently, Miach is mentioned as very much alive): 'This is what used to put fire into the warriors who were slain there, so that they were swifter on the morrow. Because Dian Cecht, his two sons Octriuil and Miach, and his daughter Airmed were singing spells over the well named Slaine. Now their mortally wounded men were cast into it as they would be slain. They were alive when they would come out. Their mortally wounded became whole through the might of the chant of the four leeches who were about the well.' In §126 Goibniu the smith himself was so healed, and 'another name for that well is Loch Luibe, for Dian Cecht used to put into it one of every herb (*lub*) that grew in Erin'.

These stories help to put some flesh on the bones of such Gaulish spa-oriented healing deities as the 'Apollo' whom Caesar claims *morbos depellere*, under various names like Bormo or Grannus (*Aquae Granni* = Aix, Aachen).[29] But otherwise there is here a rambling presence of prosthetic devices, vein-fitting, on-the-spot healing of head wounds, herbs, healing waters, and spells. In brief, all three medicines of the *Vidēvdāt* are in full legendary bloom. As we have seen, they probably belong to the warrior class and the third estate; in Celtic terms, they would not be druidic in origin. Amputation and its reverse, the attachment of limbs and setting of bones, are typical 'warrior-medicine'; even so Cuchulainn, with his second-function characteristics, was alone able to cure the damage which he inflicted. Third-estate medicine, on the other hand, would use herbs, healing liquids, and spells.

Dumézil, in his 'scapegoat period', compared the slaying of Miach to the Śunaḥśepa story and saw traces of a human sacrifice connected with royal reinvigoration and consecration (the Indic *rājasūya-* and the reinstatement of Nuadu respectively).[30] I submit instead that we witness in these Celtic traditions both the tensions and the syncreses of 'warrior-medicine' and third-function healing. In the earlier stratum Dian Cecht's prosthetic cure of Nuadu strikes Miach as evil, because it negates the healing powers which he possesses in favor of a mechanical (albeit marvelous) device. His own method, which offends Dian Cecht, involves vein-fitting and wound-healing by spells and herbs. These procedures were a stock-in-trade of popular medicine and are well attested in many parts. In much of Europe they are current folkloristic material which occasionally appears embedded in petrified mythic tradition. The best-known example is the Second Merseburg Charm (nine to tenth century): 'Phol and Wodan went to the forest. Then Balder's horse sprained its foot. Then Sinhtgunt the sister of Sunna charmed it, then Frija the sister of Volla charmed it, then Wodan charmed it, as well he could. Be it bone-sprain, be it blood-sprain, be it limb-sprain: Bone to bone, blood to blood, limb to limb, thus be they fitted together (*Ben zi bena, bluot zi bluoda, lid zi geliden, sose gelimida sin*).' The 'historiola' to which allusion is most often made in Germanic folklore versions is that of a horse (usually Christ's) breaking a leg and having it healed. Another famous attestation is found in *Kalevala* 15.307–76, where Lemminkäinen's mother pieces together the bits of her son which she has raked up from the River Tuonela: 'Flesh to flesh she joined, and bound bones to bones, members to members, veins to the ends of veins (*Liitteli lihat lihoihin, luut on luihin luikahutti, jäsenet jäsenihinsä, suonet suonten sortumihin*)', topping it off with a magic charm addressed to Suonetar 'Vein Goddess', which details the healing procedures further. In *AV* 4.12, dealing with the medicinal plant *arundhatī́*, there are close parallels, e.g. verse 4: 'Let marrow (*majjā́*) be put together with marrow, let skin (*cárma*) grow with skin, let your blood (*ásṛk*), bone (*ásthi*) grow, let flesh (*māṅsám*) grow with flesh.'[31] This particular charm, then, is of great antiquity in the Indo-European area and in contiguous traditions and may be securely assigned to the joint store of third-estate herb-and-spell medicine.

We may now attempt a summary and tentative inventory of the varieties of Indo-European mythical healing traditions, as follows:

Priests:

'Varuṇaic' prayer-healing, in reparation of inflictions by the magical sovereign in reprisal for breaches of cosmic and religious order.

'Mitraic-Aryamanic' healing, 'charters' by the legalistic sovereigns in establishment of rituals of cure.

Warriors: 'Rudraic' healing of violent afflictions, such as epidemics and mayhem.

Third Estate: Healing with herbs, liquids, and spells, in brief, folk medicine at large. Such practices are largely submythological and are attested also with such figures as the Lithuanian Mẽdis 'Wood' and the Slavic Vila.

Notes

[1] *Ormazd et Ahriman. Leurs origines et leur histoire* 293 (Paris).

[2] E.g. H. Hirt, *Die Indogermanen* 545 (Strassburg, 1907); Schrader-Nehring, *Reallexikon der indogermanischen Altertumskunde* 59 (2nd ed., Berlin, 1917).

[3] *Revue de l'histoire des religions* (*RHR*) 130.5–12.

[4] Written upon reading a (then as yet) unpublished work by J. Filliozat, presumably *La doctrine classique de la médecine indienne. Ses origines et ses parallèles grecs* (Paris, 1949).

[5] Esp. Benveniste, Symbolisme social dans les cultes gréco-italiques. *RHR* 129.5–16 (1945); Dumézil, 'Tripertita' fonctionnels chez divers peuples indo-européens. *RHR* 131.53–72 (1946); Dumézil, Triades de calamités et triades de délits à valeur trifonctionnelle chez divers peuples indo-européens. *Latomus* 14.173–85 (1955).

[6] An addition to this corpus may be seen in *Rigvidhāna* 1.2.3 (transl. J. Gonda, Utrecht 1951): 'A kṣatriya shall overcome misfortunes that have befallen him by the strength of his arms, a vaiśya and a śūdra by their wealth, the chief of the twice-born by muttered prayers and burnt-offerings.'

[7] Benveniste proscribes the version in *Yašt* 3.6 as containing 'two worthless additions', and finds a 'less precise' Rigvedic variant in 1.112.8. It is curious that the older texts should contain the less usable variants.

[8] Such as the realm of philosophy at large discussed by e.g. A. B. Keith, Greece and the philosophy of India. *The Religion and Philosophy of the Veda and Upanishads* 601–13 (*H.O.S.* 32, Cambridge, Mass., 1925).

[9] See e.g., Filliozat, *op. cit.* 161–95.

[10] A. Götze, *Zeitschrift für Indologie und Iranistik* 2.60–98, 167–77 (1923).

[11] *L'Iran sous les Sassanides* 419–20 (2nd ed., Copenhagen, 1944).

[12] Such as the data advanced by Dumézil, Les trois fonctions dans quelques traditions grecques. *Hommage à Lucien Febvre* 2.25–32 (Paris, 1953); *Aspects de la fonction guerrière chez les Indo-Européens* 93–8 (Paris, 1956); F. Vian, La triade des rois d'Orchomène: Étéoclès, Phlégyas, Minyas. *Hommages à Georges Dumézil* 215–24 (Brussels, 1960).

[13] Just as *Mitrá-* means 'Covenant', *Váruṇa-* most probably originally denotes 'Oath', literally 'that which confines' (*vṛ̥-*), cf. *árjuṇa-* 'shining', *dharúṇa-* 'bearing', *mithuná-* 'paired' (lit. 'interchanging'). The semantic link is similar to that between Gk. *hórkos* 'oath' and *hérkos* 'enclosure'. This interpretation comes closest to that of R. Hiersche, Zur Etymologie des Götternamens Varuṇa. *Mitteilungen des Instituts für Orientforschung* 4.359–63 (Berlin, 1956); *cf. Revue des études grecques* 71.35–41 (1958). P. Thieme's rendering 'True Speech' (e.g. *Mitra and Aryaman* 61 [1957]) goes ultimately back to Meillet (*Journal asiatique* Sér. 10, Tome 10, 157 [1907]) and involves the same root **wer-* 'speak' (including *vratá-*), while staying

within the semantic range of 'oath'. E. P. Hamp, *Indo-Iranian Journal* 4.64–5 (1960), supports it derivationally, while it is criticized by W. P. Schmid, *Kratylos* 7.137-39 (1962). Otherwise the multiple Varuṇa-etymologies, now conveniently summarized in M. Mayrhofer, *Kurzgef. etym. Wb. des Altind.* 3.151–53 (Heidelberg, 1967), continue to proliferate. Of the latest, mutually exclusive connections by F. R. Schröder and R. Jakobson, the former (*PBB* [T] 84.1–4 [1962]) brings in the Old Norse giant *Vǫrnir* as **Warunjaz* (also *úraṇa-*, Lat. *vervex* 'ram'), while the latter (Mayrhofer, *op. cit.* 152) revives a hunch by De Saussure in a letter to Meillet (now published in *Cahiers F. de Saussure* 21.115-16 [1964]) and ties in Lith. *vẽlės* 'Manes', *vẽlinas, vélnias* 'devil', *Veliuonà* 'goddess of the Manes', the Old Russian deity *Velesŭ*, etc. Cf. *Studi linguistici in onore di Vittore Pisani* 579–99 (Brescia, 1969).

[14] The dual *ródasī* 'heaven and earth' is elliptical, like *dyávā* for *dyávāpṛthivī*; see M. Mayrhofer, Der Gottesname Rudra. *ZDMG* 103.140–50 (1953).

[15] Cf. e.g. L. Honko, *Krankheitsprojektile* (*FFC* 178, 1959) 40–82, esp. 62–65.

[16] H. Grégoire, R. Goossens, M. Mathieu, *Asklépios, Apollon Smintheus et Rudra. Études sur le dieu à la taupe et le dieu au rat dans la Grèce et dans l'Inde* (Académie Royale de Belgique, Classe des Lettres, Mémoires 45.1, Brussels 1949), esp. 127–73. Cf. Goossens, *La Nouvelle Clio* 1.4–22 (1949).

[17] Cf. L. Renou, Note sur les origines védiques de Gaṇeśa. *Journal asiatique* 229. 271–74 (1937), where the elephant-symbolism is traced back all the way to the *Yajurveda* (*MS*).

[18] The interpretation of *Nâsatyā* as 'Saviors' by H. Güntert and others, never very satisfactory formally and connectible specifically with healing only through a rapprochement with specific Germanic semantic developments (Goth. *ganisan* 'recover' < 'return', *nasjan* 'save' < 'cause to return'), has been further weakened by H. Lommel's rather persuasive revival (*Festschrift Walther Schubring* 29–31 [Hamburg, 1951]) of Yāska's (*Nirukta* 6.13) discounted etymology *nāsā+tya* 'nose-born', with reference to the myth of the coupling of Vivasvant and Saraṇyū in horse form and the latter's sniffing of the spilled sperm which led to the birth of the Aśvins (*Bṛhaddevatā* 6.162–7.7; cf. *ni-tya-* 'oikeîos', *amá-tya-* 'súnoikos', *āp-tyá-* 'énudros', etc.).

[19] In addition to the discussed passage *RV* 10.39.3, the tendency to a formal universalization of the healing function of the Aśvins is seen in *RV* 1.34.6: *trír no aśvinā divyáni bheṣajá tríḥ pārthivāni trír u dattam adbhyáḥ* 'thrice, Aśvins, grant us the medicines of heaven, thrice those of earth, and thrice those of the waters'. Certain authors have latched on to Sāyaṇa's hindsight of nearly two millennia and tried to see here the origins of the Ayurvedic (and Platonic) doctrine of the threefold humoral origin of disease discussed earlier (breath, phlegm, bile; justly rejected by R.F.G. Müller, *Asia Major* 6.335–6 [1930]). In reality the whole hymn turns on the phrase 'thrice' which has here largely stylistic relevance. The only noteworthy feature is the mention of 'waters' with medicinal intent, rather than some such stereotyped third component as *antárikṣam* to make up the whole 'universe'.

[20] The story of Viśpalā (*RV* 1.116.15 *carítraṁ hí vér ivácchedi parṇáṁ ājá khelásya páritakmyāyāṁ/sadyó jáṅghām áyasīm viśpálāyai dháne hité sártave práty adhattaṁ* 'for like a bird's pinion a leg was severed at the height of Khela's race; at once you [i.e. Aśvins] fitted an iron shank to Viśpalā, to run for the posted trophy') seems further to record the orthopedic prowess of the Aśvins. R. Pischel long ago demoted Viśpalā from a woman to a mare (Pischel-Geldner, *Vedische Studien* 1.171–73 [Stuttgart, 1888]), and Dumézil has more recently tried to integrate the equine Viś-palā mythologically with the mutilated horse of the Roman Pales-festival (*Eranos* 54.232–45 [1956]), which would point to an Indo-European origin. The quasi -surgical activity of bone-setting is ascribed to the *bhiṣák* in *RV* 9.112.1 (*rutám bhiṣág . . . icchati* 'seeks out what is fractured'). These instances prove neither that the Aśvins were basically concerned with surgical matters nor that this specialization inheres in the Rigvedic meaning of *bhiṣáj-*. In view of Avestan *biš-* 'cure', *-biš-* 'healing', the etymology is either Indo-Iranian **bhiš-áź-* (with suffix) or **bhiš-sáź-* ('seeking cures', IE **saĝ-*, with simplification of intervocalic *-šs-*, as suggested by P. Thieme, *Festschrift*

Schubring 10). Any root-connection of **bhiš-* is conjectural; a reconstruction **bhəs-* (**bhā-s-* in Skt. *bhāṣ-* 'speak') is weak on the Iranian side and would indicate the semantic primacy of verbal cures.

[21] Pischel-Geldner, *Vedische Studien* 3.71–2 (Stuttgart, 1901).

[22] *RV* 1.181.4 *jiṣṇúr vām anyáḥ súmakhasya sūrír divó anyáḥ subhágaḥ putrá ūhe* means 'one of you is the victorious offspring of Sumakha, the other is held to be the blessed son of heaven', rather than 'one of you is the master of Sumakha (= Rudra) . . . '. Further, *rudrávartani-* means 'heaven-ranging' as a standard epithet of the Aśvins and has no relevance to any competition with Rudra.

[23] See *Pahlavi Texts* translated by E. W. West, *Sacred Books of the East* 11.125–6 (1901).

[24] *RV* 1.139.7 *yád dha tyắm aṅgirobhyo dhenúṁ devā adattana/ví tāṁ duhre aryamā́ kartárī sácā* . . . 'ever since you gave that cow to the Angirases, gods, they have milked her, Aryaman along with the officiator'.

[25] *cumdach tochair* . . . *bōthair, rōd rīgraidi*; see *Lebor Gabála Érenn*, Part IV, ed. R.A.S. Macalister, 262–5 (Irish Text Society, vol. 41, Dublin 1941). This aspect of Aryaman-Eremon might ultimately shed light on the specific character of Lat. *pontifex* as well, as a specialized development of the general notion seen in Ved. *pathikṛ́t-*.

[26] Jan de Vries (*Cahiers du Sud* 36.18–27 [1952]; *Altgermanische Religionsgeschichte* 2.16 [2nd ed., Berlin 1957]) has revived the connection of the Germanic *Irmin-* (theonymic or theophorous in *Irminsul, Ermanaric, Iömungandr*, etc.) with Skt. *Aryamán-* (cf. OSax. *irmintheod* 'mankind') in the sense of 'belonging to the people, one's own' (agreeing with the sense established by Dumézil for Skt. *aryá-* and by Laroche for Hittite *ara-*; see *Hommages à Georges Dumézil* 124–8 [Brussels, 1960]). De Vries also connects *Irmin* closely with *Tīwaz, Týr*, thus the Mitra-type of sovereign (cf. *Hommages à Georges Dumézil* 92 = *Kleine Schriften* 158 [Berlin, 1965]). Thereby the Western Indo-European attestation of the Mitra-Aryaman complex is further underscored.

[27] *Lebor Gabála* 7.329 (Part IV, ed. Macalister, 148–9); cf. also 7.310, *ibid.* 112–5.

[28] Wh. Stokes, *Revue celtique* 12.52–130 (1891).

[29] Cf. e.g. Jan de Vries, *Keltische Religion* 71–78 (Stuttgart, 1961); F. Le Roux, *Ogam* 12.59–72 (1960).

[30] *Flamen-Brahman* 78–80 (Paris, 1935).

[31] The Second Merseburg Charm and *AV* 4.12 were first compared by A. Kuhn in Indische und germanische Segenssprüche. *KZ* 13.49–74 (1864). Of the later immense literature we may cite R.Th. Christiansen, *Die finnischen und nordischen Varianten des zweiten Merseburger Spruches* (*FFC* 18, 1915); F. Genzmer, Die Götter des zweiten Merseburger Zauberspruchs. *Arkiv för nordisk filologi* 63.55–72 (1948); F. R. Schröder, Balder und der zweite Merseburger Spruch. *Germanisch-romanische Monatsschrift* 34.161–83 (1953); B. Schlerath, Zu den Merseburger Zaubersprüchen. *II. Fachtagung für Indogermanische und Allgemeine Sprachwissenschaft, Innsbruck, 10.–15. Oktober 1961*, 139–43 (= *Innsbrucker Beiträge zur Kulturwissenschaft*, Sonderheft 15, 1962); E. O.G. Turville-Petre, Myth and Religion of the North 122–5 (New York, 1964).

Is the 'Kingship in Heaven' Theme Indo-European?

C. Scott Littleton

Some years ago, the eminent Swedish Iranianist Stig Wikander (1951, 1952) sought to demonstrate that the widespread ancient theogonic theme commonly referred to as the 'kingship in heaven'[1] was rooted in the common Indo-European speaking mythological tradition and not, as most scholars believe, indigenous to Mesopotamia. If correct, the importance of Wikander's thesis cannot be overestimated, for despite a century of research from almost every conceivable angle (naturism, rationalism, ritualism, even psychoanalysis) into the nature of Indo-European mythology, to say nothing of the major advances that have been made in the last several decades by Dumézil and others in the reconstruction of a common Indo-European ideology, no clear picture of how the Proto-Indo-Europeans viewed the origin of their world and the gods who rule it has yet come to light. The purposes of this paper[2] thus are (1) to reexamine briefly the evidence[3] used by both Wikander and those who argue for a non-Indo-European origin, (2) to put into evidence three heretofore unrecognized versions of the theme (or at least unrecognized by Wikander), and (3) to reach some tentative conclusions as to the validity of Wikander's position.

The Greek, Hittite-Hurrian, Phoenician, and Iranian Versions of the 'Kingship in Heaven'

Traditionally, scholars have recognized two major versions of the 'kingship in heaven': the Greek theogony, as delineated by Hesiod, Apollodorus et al., and the Phoenician account of the parentage of Ba'al, as recounted by Herrenios Philo of Byblos in his *Phoenician History* (cf. Clemen 1939). In the 1930's, as the Hittite-Hurrian texts came to be fully translated, Forrer (1936) and Gütterbock (1945, 1948, 1952) and others were able to delineate an ancient Anatolian version. More recently, Wikander was able to demonstrate its presence in the *Shāhnāmeh* of Firdausi and thus to include Iran within the circle of regions knowing the theme. In the pages that follow we shall attempt to demonstrate its presence in the Norse accounts of Ymir and his offspring, as recorded by Saemundar and Snorri, and in two distinct Babylonian accounts of the creation: the well-known *Enūma Elish*[4] and the newly discovered 'Theogony of Dunnu' (Lambert and Walcot 1965).

In each of the above instances, a single theme is present:[5] an existing generation of gods was preceded by two (in some cases three) earlier generations of supernatural beings, each succeeding generation being presided over by a 'king in heaven' who has usurped (or at least assumed) the power of his predecessor Moreover, there is generally a fourth figure, a monster of some sort, who, acting on behalf of the deposed 'king' (in the Iranian and Norse versions, as we shall see, the monster became identified with the deposed 'king' himself), presents a challenge to the final heavenly ruler and must be overcome before the latter can assert full and perpetual authority.

By all odds the best known and most heavily documented version of the 'kingship in heaven' is that of the ancient Greeks, and, although it is far from the oldest—the Hurrian-Hittite version antidates Hesiod by at least 700 years, and the Babylonian versions date back perhaps to 2000 B.C.—we shall use it for the sake of convenience as a sort of model in terms of which we can view the others. The broad outlines of the Greek theogony are indeed well known. The chief primary sources are the *Theogony* of Hesiod (ca. 700 B.C.), the *Bibliotheca* of Apollodorus (ca. 100 B.C.), and the *Dionysiaca* of Nonnos, which despite its fifth century A.D. date, includes some original materials not found elsewhere among classical works on myth.

In the beginning there is naught but Chaos. Eventually the autochthon Gaia or Earth makes her appearance and gives birth to the first heavenly king Ouranos, who becomes her husband. The second generation includes a host of creatures (e.g. Okeanos, Theia) born of this primal pair. The youngest offspring is Kronos, who, with the encouragement of his mother, castrates Ouranos with a stone sickle and thereby renders him powerless. After this act of emasculation Kronos banishes his father to Tartaros, frees his siblings from bondage, and sets himself up as the second 'king in heaven.' Espousing his sister, Rhea, Kronos produces a set of offspring, the youngest of whom is Zeus. Seeking to prevent a repetition of the events that brought him to power, Kronos devours each of his children as soon as they are born. All, that is, save for Zeus, whose life is spared by a ruse. Rhea gives her husband a stone wrapped in swaddling clothes to swallow and is able to hide the infant Zeus away in Crete, where he grows to manhood. Eventually, Zeus returns and deposes Kronos, giving him an emetic that causes him to vomit up all of the swallowed children. A war ensues between the gods (i.e. Zeus and his siblings) and the Titans (i.e. Kronos and others of his generation). Kronos is eventually defeated and consigned to Tartaros along with those who had supported him in the war. An alternate tradition, reflected in Pindar's 12th *Olympic Ode*, sees Kronos' fate as banishment to the 'Isles of the Blessed', somewhere in the Atlantic (perhaps Britain?). In any event, Zeus now reigns as the third 'king in heaven.'

In order to validate his newly won position, Zeus must overcome a final challenge to his authority in the person of the monstrous figure Typhon. Born in Cilicia of a union between Earth and Tartaros (*Bib.* I.4.2; *Theog.*

820), Typhon is described as a giant standing in the sea with 'his feet firm on the weedy bottom, his belly in air, and [his head?] crushed in the clouds' (*Dion.* I.266–268). Zeus engages him in single combat and manages to defeat him, though only after a heroic effort, and some timely assistance on the part of Hermes.[6]

With Typhon defeated, Zeus reigns triumphantly, and the subsequent events in his divine career fall outside the scope of the theme in question.

These events are duplicated in the account given by Philo of Byblos (first century A.D.) of the mythical origins of his city.[7] Even the names in many instances are similar, and the Phoenician version has often been regarded as intermediate between that just discussed and that of the Hurrians and Hittites (cf. Gütterbock 1948:34). Here, the first heavenly king is Eliun (or Hypsistos, 'Highest'), who, with his wife Bruth (i.e. Beirut), comes to live in Byblos (*Philo* I.14). They give birth to a son called Ouranos and a daughter named Ge or Gaia. These two marry and produce four sons: El (who is also referred to as Kronos), Baitylos, Dagon, and Atlas. Ouranos tries to destroy his children, but El forges a sickle (or spear?) and drives out his father. Marrying three of his sisters, Rhea, Astarte, and Dione, he assumes the kingship. In due course he fathers a son called Ba'al. Thirty-two years after his accession, El lures his father back to Byblos and castrates him (cf. *Theog.* 175). Thus, castration is present, albeit delayed, a situation we shall also encounter shortly in the Iranian version.

Eventually, the fourth generation, in the person of Ba'al, takes over the kingship. This transfer of power is apparently accomplished without much conflict (Clemen 1939:28), an occurrence unique in the distribution of the theme. Typhon is mentioned by Philo along with the children of El, but there is no mention of a fight. Moreover, the role of the Zeus-figure (i.e. Ba'al) is minor when compared to that played by him in the other versions.

In view of its late date and the high probability that its author was thoroughly familiar with Hesiod, a great many scholars have been skeptical of this Phoenician 'theogony', labeling it a poor attempt at syncretism. However, the discovery of some Hurrian texts at Ras Shamra (cf. Gütterbock 1948:133), wherein the double name El-Kumarbi occurs—Kumarbi is clearly the Hurro-Hittite Kronos-figure—has thrown a new light on the matter. As El is identified by Philo with Kronos, it can be reasonably inferred that there was some sort of Kronos-El-Kumarbi syncretism present in northern Syria, at least, as early as 1400 B.C. If this is correct, then it is also quite reasonable to infer, with Güterbock, that the Phoenician tradition just discussed forms a link between the version known to the Hurrians and Hittites and the later Greek version, and that what formerly appeared as rank syncretism on Philo's part can now be seen as antedating rather than reflecting the Hesiodic version of the theme.[8] But as we shall see later on, there remain still other possibilities.

Of far greater antiquity than the versions of the 'kingship in heaven' just discussed is that contained in a series of cuneiform tablets unearthed near the modern Turkish village of Bogazköy, site of the ancient Hittite

capital Hattusha. Dating from the thirteenth century B.C., these tablets are written in the Indo-European *nesili* language. Yet the presence of a great many Hurrian god names (see below) leads to the inescapable conclusion (cf. Güterbock 1946, 1948) that the Hurrians, too, knew the theme in question and that perhaps it was they who passed it along to their Indo-European speaking neighbors. It is for this reason that we refer to this version as the 'Hittite-Hurrian' version.

These tablets are not well preserved and a great many interpolations have had to be made in order to arrive at anything like a coherent narrative. For our purposes two texts are especially important: that labeled by Güterbock the 'Theogony' (the Hittite title is unfortunately missing) and the 'Song of Ullikummi.' The former deals specifically with a pattern of divine succession that remarkably parallels the one delineated by Hesiod et al. Four generations of gods are mentioned. The first is called Alalu, who reigns in heaven nine 'years' (more probably 'ages' or 'eras'; cf. Güterbock 1948:124). He is succeeded by his son[9] Anu (cf. the great Sumerian sky god An), who in the ninth 'year' rebels against his father and takes possession of the kingship. He in turn is overthrown by a rebellious offspring, the 'mighty Kumarbi.' The name Kumarbi is clearly Hurrian and is usually equated with the great Sumero-Akkadian god Enlil in Hurrian texts (Güterbock 1961:160). Again, after nine 'years', Kumarbi overthrows Anu. This time, however, the elder god does not give up so easily. He flees skyward, but is pulled down by Kumarbi, who then bites his 'loins' (i.23–5). The Hittite word in question here is *pár-ši-nu-uš-šu-uš*, which can be explained as *parsnus-sus* 'his hips or loins' and is clearly meant as a euphemism for genitals. Thus, in biting Anu's genitals, Kumarbi, like Kronos, performs an act of castration, only in this instance, as we shall shortly see, it has a direct bearing on the birth of the final divine ruler.

Subsequent to his deposition and emasculation, Anu informs Kumarbi that by swallowing his 'manhood' he has been impregnated with five 'heavy' divinities: the Weather-God (i.e. the Hurrian Teshub),[10] the river goddes Aranzakh (i.e. the Tigris), Tashmishu, who is destined to be the vizir of the gods, and two others whose names are not mentioned by Anu.[11] From here on the text is too fragmentary for a consecutive narrative. It appears that Kumarbi gives birth to these gods in some miraculous fashion.[12] Especially miraculous is the birth of the Weather-God or Teshub (cf. the attention devoted by the Greeks to the birth of Zeus, to whom, as we shall see, Teshub corresponds in most respects). Then follows a fragmentary passage wherein someone, seemingly Kumarbi, says 'give me the child . . . I shall eat' (i.42). Later there occurs the expression 'Kumarbi begins to eat' (i.52) and prominent mention is made of the words 'mouth' and 'teeth' in connection with the Weather-God. Eventually, however, Teshub and his siblings are able to avoid being eaten and to dethrone Kumarbi, for when we first meet them, the Weather-God is already 'king in heaven.'[13]

Like Zeus, Teshub, too, must overcome a final challenger. In this instance it is the monstrous figure Ullikummi, and the relevant text is the previously mentioned 'Song of Ullikummi'.

After his deposition, Kumarbi has intercourse with a rock. From this unnatural union is produced the stone-monster Ullikummi, whose purpose in life is to avenge his father's overthrow. (That Ullikummi's Greek counterpart, Typhon, has a similar purpose is clearly evident; cf. *Dion.* II.565–68.) The account of Teshub's conflict with the Stone-Monster is quite extended, and we do not intend to discuss it in detail here (see Güterbock 1948:127; 1961:164–72; Goetze 1955:121–5, summarized by Littleton in press: 14–16). Nevertheless, a few salient descriptions and incidents should be mentioned for their comparative value. First of all, the description of Ullikummi is almost identical to that which Nonnos gives of Typhon: 'Out of the water he stood, the Stone . . . and up in Heaven the Temples and the chamber he reached' (I.23–32). Cf. Typhon with his feet in the water, his belly in the air, and his head 'crushed' against the clouds. Second, the combat between the two occurs in the vicinity of the same mountain (i.e. Mt. Ḫazzi = M. Kasios, the modern Kel dağ or 'Bald Mountain', located just south of the mouth of the Orontes River in what is now Turkey). Third, in order to gain the wisdom necessary to defeat the monster, the gods, Teshub among them, visit Ea, the Babylonian god of wisdom and witchcraft (cf. the *Enūma Elish*, shortly to be discussed, wherein Ea occupies a prominent position), at his home in Apsuwa (the Babylonian Apsu). The ultimate undoing of Ullikummi is accomplished with a cutting tool of some sort. The Stone-Monster had been riding upon the shoulders of the Atlas-like Upelluri, and when he is cut free, his power rapidly ebbs. The extent to which this implement is similar to the 'adamantine' sickle used in the castration of Ouranos and by Zeus in his combat with Typhon is not wholly clear, but the evidence here is certainly suggestive of correspondence.

With Ullikummi defeated, Teshub, like Zeus and Ba'al, reigns as perpetual 'king in heaven'. One major difference between the Hurrian-Hittite and Greek versions is that the conflict between Teshub and Ullikummi is less clearly a matter of single combat. It appears that the gods as a group, albeit under the leadership of Teshub, are involved. Nevertheless, the basic confrontation here is between the final 'king in heaven' and the last hope of his deposed predecessor. In this the two accounts agree perfectly.

It was, as we have said, Wikander (1951) who first demonstrated the presence of the 'kingship in heaven' in the Iranian tradition. Here, the primary source is Firdausi's *Shāhnāmeh*, or 'Epic of Kings', composed about 976 A.D. Despite the relatively recent date of its composition and the high probability that its author was familiar with Greek myth, the *Shāhnāmeh* has long been considered to contain popular traditions not elsewhere encountered in Iranian literature. This would certainly appear to be true as far as the theme in question is concerned, although the extent to which its presence here, as Wikander suggests, necessarily implies an Indo-European origin is very much open to question. As the Iranian version of the divine kingship is in large measure the pivot, so to speak, upon which Wikander's theory turns, it is necessary for us to examine it in more detail than has been necessary heretofore in our survey.

The three Iranian kings cited by Wikander as comparable to Ouranos, Kronos, Zeus et al., are Jamshid, Zohak, and Feridun, who occupy, respectively, positions four, five, and six in Firdausi's king list. Jamshid is preceded by three relatively indistinct figures, Kaiumers (equals Gayomart in the *Avesta*), Husheng, and Tahumers. These three do not seem to be related to their successors in any important sense—Jamshid is made the son of Tashumers, but little else is said about the relationship between them. Thus, it is Jamshid who occupies the Ouranos-like position here, despite his lack of an autochthonous or truly divine origin.

Jamshid, whose name is equivalent to that of Yima[14] in the *Avesta*, is said to have ruled for some 700 years, and the early portion of this reign is described as a sort of golden age, an age when men were at peace with one another and the land was bountiful. However, this state of affairs did not last. 'Then it came about that the heart of Jamshid was uplifted with pride, and he forgot whence came his weal and the source of his blessings' (Zimmern 1926:4). Wikander, in discussing the position of Jamshid notes that he 'règne d'abord sur une humanité heureuse, mais il commet ensuite le premier péché ce qui amène la perte de la Gloire Royale et sa chute' (Wikander 1952:13). He also points out that some texts show Jamshid as having been deceived by 'une figure féminine qui aurait inspiré ses transgressions et causé sa chute.' Thus, we have some indication that here, too, there is a Gaia-like figure somewhere in the background (cf. Wikander 1951:41).

Jamshid is eventually overthrown by Zohak (equals Azi Dahāka in the *Avesta*), who, in terms of our model, occupies an ambiguous position. For he is at once Typhon and Kronos. Both in his physical appearance and in his relationship with the third member of the trio, Feridun, who occupies the position of Zeus-figure, Zohak strongly resembles Typhon. Yet he enters the epic occupying the position of a Kronos-figure. Like Typhon, his physical appearance is characterized by the presence of snakes growing from his shoulders (cf. Atkinson 1832:34); yet he is the one who overthrows Jamshid and who commits the inevitable act of mutilation, although in this instance it is not castration but rather a sawing in half.[15]

There are two interesting parallels here to the Phoenician version. Like El, Zohak delays the mutilation of his deposed enemy: 'in the hundredth year [after his overthrow] the impious shah [Jamshid] appeared one day beside the sea of Chin. Zohak clutched him forthwith, gave him no respite, and, sawing him asunder, freed the world from him and the fear that he inspired' (Warner and Warner 1905:140). A second parallel can be seen in the fact that Zohak, like El, marries two sisters who stand in a close kinship to him—only in this instance they are sisters of the deposed first generation ruler (Jamshid).

Wikander (1952:13) emphasizes the similarity between the saw that cut Jamshid in half and the sickle that castrated Ouranos. He also characterizes Zohak as the 'neveu' of Jamshid, although nowhere in our reading of the epic is there any clear-cut statement as to the relationship, if any,

between the two figures. Zohak is characterized simply as the son of an Arabian king (cf. Warner and Warner 1905:141) who is invited to come to Iran and replace Jamshid.

Zohak, in his turn, is overthrown by the grandson of Jamshid, Feridun (equals Thraetaona in the *Avesta*) who, like Zeus, has a marvelous childhood. Zohak, it appears, while having no offspring of his own, has an insatiable desire to consume human beings. Furthermore, the Serpent-King dreams that Feridun will someday overcome him (cf. Kronos' foreknowledge of Zeus' coming, and his subsequent swallowing of his offspring) and on the basis of this 'bade the world be scoured for Feridun' (Zimmern 1926:8). Feridun's mother first places him in the care of a wondrous cow, Purmaieh, who suckles the infant. Fearing that Zohak will find this hiding place, she removes him to the care of a shepherd 'on the Mount Alberz' (i.e. the Elbruz?), who raises him to manhood. Here we can compare the hiding away of Zeus on Crete. When Feridun grows to manhood, he sets out to overcome his monstrous enemy. Instead of a cutting implement, Feridun uses a club, the head of which is shaped like a cow—in memory, perhaps, of Purmaieh. The combat between Feridun and Zohak is strongly reminiscent of the conflict between Zeus and Typhon. Again, it is a single combat situation, not the type of group action which dethroned Kronos.

Feridun overpowers Zohak, but does not actually kill him (a fusion, perhaps, of the Typhon and Kronos motifs); rather, he chains the monster to a rock[16] on Mount Demawend (?), and Zohak eventually dies of exposure. After this deed is accomplished, Feridun reigns as king, if not actually 'in heaven', then certainly as a divine king in Iran.

While it is certain that the Avestan names Yima, Azi Dahāka, and Thraetaona correspond, respectively, to Jamshid, Zohak, and Feridun, the only clear thematic parallels between the two sets of figures are that both Yima and Jamshid can be seen to be in one respect or another primordial (although Jamshid does indeed have forebears), that Azi Dahāka and his later namesake share draconic characteristics, and that both are rendered harmless, respectively, by Thraetaona and Feridun. Even in the latter instance there is an important difference. Thraetaona is characterized as the 'smiter' of Azi Dahāka,[17] while Feridun, as we have seen, imprisons Zohak. Thus, it can be safely asserted that the theme qua theme, despite a few isolated similarities, is not present in the *Avesta*.

Nor is it to be found in the Indic tradition. To be sure, there are some very general parallels, both thematic and philological, between the Avestan figures mentioned above and some of the dramatis personae of the Indic literature. For example, Avestan Yima parallels Vedic Yama; Azi Dahāka is equivalent to the three-headed monster Vṛtra (cf. also Indra in this context); and the cow-headed weapon (*gurz*) used by Feridun is linguistically cognate to the *vajra* used in the slaying of Vṛtra. But other than these isolated instances, together with one other possibility to be mentioned shortly in connection with the Norse version, there is no evidence for the presence of the theme in question in the *Vedas*, *Mahābhārata*, etc.[18] This

negative evidence, so to speak, is a most important matter when it comes to the question of possible Indo-European origins, and we shall return to it later on.

Having surveyed the basis upon which Wikander has constructed his theory of an Indo-European origin, our next task is to put into evidence three additional versions of the 'kingship in heaven' not considered by him: that contained in the Norse literature, the *Enūma Elish*, and the 'Theogony of Dunnu'.

The Norse Version

All too often there is a tendency among students of comparative mythology to equate 'Norse' with 'Germanic', to assume that the materials contained in the *Eddas*, *Heimskringla*, *Gesta Danorum*, etc., are a true reflection of common Germanic beliefs and religious practices. To make such an equation is, of course, an error; for it is clear that there were differences in religious outlook among the several branches of the Germanic speaking peoples. One need only compare Tacitus with Saxo Grammaticus, to see examples of these differences. While there are some common figures (e.g. **Tiwaz*, probable prototype of Norse *Týr* and Anglo-Saxon *Tiw*), any attempt to draw general conclusions about the nature of Germanic religion from any one region or era must thus be made with caution.

Yet when it comes to cosmogonic and theogonic matters we are necessarily limited to a single region and a single era: Scandinavia (primarily Iceland) in the eleventh and twelfth centuries A.D. Moreover, our chief sources, the *Elder* or *Poetic Edda*, attributed to Saemundar Siggfusson, and the *Younger*, or *Prose Edda* of Snorri Sturlson, were composed at a time when the old religion was fast giving way to Christianity (Snorri approaches his subject from an explicitly Christian standpoint), and the extent to which non-Norse materials were interwoven with the native tradition is still not wholly clear.

That Saemundar and Snorri were aware—albeit dimly—of the mythic traditions of the Eastern Mediterranean is entirely possible.[19] It is also possible, as we shall suggest later on, that whatever parallels may in fact exist between their accounts of the creation and those previously discussed in this paper are wholly the result of independent invention. In any case, all of the foregoing must be kept firmly in mind as we proceed to a brief examination of the Norse theogony to see if it contains materials relevant to the 'kingship in heaven'.

In the beginning was *Ginnungagap* (equivalent to the Greek *Chaos*), which can be loosely translated as 'yawning void'. Out of this yawning void are created initially two regions: *Muspellheim* on the south (i.e. a 'Land of Fire') and *Niflheim* on the north (i.e. a 'Land of Mist'). Ice crystals combine with sparks, and out of this combination there is created the first being, called Ymir. Ymir, defined as a giant of great proportions, lies down and sleeps. While he sleeps two things happen: first, a second authochthonous creature appears, a cow named Audhumla. While Ymir sleeps the cow

nourishes him, and at the same time licks the salty ice and slowly uncovers first the hair, then the body of a third autochthon, Buri. Thus, Audhumla serves as a link between these two generationally equivalent *Jöntin* (occasionally rendered *Etin*), or 'Giants'. Meanwhile, in his sleep, Ymir androgynously gives birth to several offspring: (*Lay of Vafthrudhnir* 35)[20] 'The ice-etin's [i.e. Ymir's] strong arms, beneath there grew both girl and boy, one with the other, the wise etin's shank begat a six headed son.' The latter is named Thrudhgelmir. He, in turn, gives birth to the crafty Bergelmir. Thus, we have one three-generation line of descent from Ymir. As the *Lay of Vafthrudhnir* (29) has it: 'Ages before the earth was made, Bergelmir came to be. Thrudhgelmir was that thur's (?) father. But Aurgelmir [i.e. Örgelmir, or Ymir] oldest of all.'

A second three-generational line descends from Buri, who, after emerging from the ice, produces a son: Bör. Bör marries a giantess named Bestla (one of Ymir's offspring?). They, in turn, produce three sons: Odinn, Vili and Ve (cf. Zeus, Poseidon, Hades). Odinn and his siblings (who unlike their Greek equivalents become shadowy figures very quickly) are the first of the *Aesir* (or 'Gods'). Their first act is to overthrow Ymir, and with him Thrudhgelmir. Bergelmir they banish (or he escapes)[21] to subterranean Jöntinheim (cf. Tartaros?). Ymir they cut up (unfortunately the tool involved is not mentioned), forming the world: '. . . of Ymir's flesh the earth was shaped, of his blood, the briny sea, of his hair, the trees, the hills of his bones, out of his skull the sky.' (*Lay of Grimnir* 40).

There is a curious parallel to this in the Rigveda 10.90, wherein Indra, et al. create the world from the flesh and bones of the giant Purusha. Another parallel can be seen in the Babylonian *Enūma Elish* (shortly to be discussed) in the use to which Marduk puts the body of the slain Tiamat. The similarities here, however, may best be explained in terms of a common folkloristic motif which has nothing specifically to do with the theme under discussion. Yet the fact that Odinn and his siblings do *cut* the primeval figure is significant. It is this rather than the subsequent use made of his remains that is relevant to our purposes.

In any event, it is only after the *Jöntin* have been defeated that Odinn and his brothers create mankind. This, of course, is paralleled by the Greek version (i.e. the human race is only created after the final defeat of the Titans; cf. *Bib.* I.4.1).

It is our contention that the events just described contain all of the essential ingredients of the 'kingship in heaven' theme as described elsewhere in this paper: First, there is a three-generational line of descent (though bifurcated); second, there is the mutilation of the first generation 'king' (i.e. Odinn's cutting up of Ymir); third, there is the banishment of a descendant of this first generation being by one who has usurped power from him (Bergelmir is the logical inheritor of Ymir, who is thwarted of his inheritance); fourth, the final and perpetual holder of power (Odinn) is, together with his siblings and offspring, defined as an altogether different sort of supernatural being (i.e. 'Gods' or *Aesir*, as opposed to 'Giants' or *Jöntin*;

cf. the Greek distinction between 'God' and 'Titan'). And finally, there is a battle between these *Aesir* and the Giants, a battle which seems to be equivalent to that between the Olympians and the Titans[22] (or Giants too, for that matter).

That the characteristic structure of the theme, as expressed in the Greek and Hittite-Hurrian tradition, is absent must be admitted. However, contained within a broad three-generational framework, most of the significant components of this structure are present. To put it another way, the same broad configuration is present in Norse mythology, however the elements within this configuration are not for the most part structured as they are in the Greek tradition. Indeed, we have seen other cases, generally held to be part of the theme in question, which also deviate from this typological structure, yet which maintain the same broad configuration. We may cite here the Iranian version, wherein Zohak, who most clearly resembles Typhon, appears in the role of a Kronos-figure. We may also cite the Phoenician case, wherein Ba'al does not have to fight his way into power, as is so characteristic of the other versions—including the Norse.

It thus seems fair to assert that the Norse, like the Greeks, Hittites, Hurrians, Phoenicians, Iranians, and, as we shall shortly see, the Babylonians, knew that which we have labeled the 'kingship in heaven' theme. Whether its presence here can best be explained in terms of diffusion or independent invention is still a moot point, although we believe that the former possibility is the more probable, given the extremely late period from which our primary sources date. There is, of course, the alternate possibility that the presence of the theme can be explained in terms of the common Indo-European heritage. Once again, let us defer consideration of this possibility—seemingly quite remote—until we have had a chance to look at the Babylonian versions.

The Babylonian Versions

Like the Norse version, the two Babylonian versions deviate in a number of important respects from that which for convenience we have labeled the typological version (i.e. that of the Greeks). Yet these two related accounts of how the gods came to be may well prove to be far closer to the source of the theme in question than any previously considered. The best-known is the *Enūma Elish*, so named from its opening words (see below). Although no extant text dates earlier than 1000 B.C., from internal evidence alone it is quite likely that its composition dates at least from the Old Babylonian period, i.e. the early part of the second millennium B.C., and that its content may perhaps be considerably older.[23]

The *Enūma Elish* begins with an account of the primeval state of things: 'When on high the heaven had not been named, /Firm ground below had not been called by name, /Naught but primeval Apsu,[24] their begetter, /(And) Mummu-Tiamat,[25] she who bore them all, /Their waters comingling as a single body . . .' (i.1–4).[26] We thus see here the by now familiar pair of autochthons, in this instance defined as fresh and salty water.

Eventually, several generations of divinities are born, including the figures (for our purposes obscure) Lahmu and Lahamu, Anshar and Kishar. Finally, there appears the figure who, together with another shortly to be discussed, occupies the Kronos position here, the 'all-wise' Ea (or Nudimmud) (= Sumerian Enki). Although the exact parentage and birth order of Ea is obscure, by all indications he is the youngest of the lot (who can, for our purposes, be reckoned as a single generation; i.e. they band together and act as a generational unit).[27] If this interpretation is correct, then there would be an interesting parallel here to the ultimogeniture pattern so clearly evident in the Greek version.

Under the apparent leadership of Ea, the gods of what we may term the second generation (see above) 'disturbed Tiamat as they surged back and forth' (i.22). Apsu, too, is annoyed by 'their hilarity in the Abode of Heaven' (i.24) and decides to do away with them. His decision is strengthened by the advice of the vizir, Mummu,[28] but is opposed by Tiamat, who counsels forgiveness. Nevertheless, the gods discover Apsu's intentions. After putting Apsu to sleep with an incantation, Ea slays him; however, before doing so he tears off the former's tiara or halo (symbolic, perhaps, of sovereignty and the masculine vigor that accompanies it) and puts it on himself (i.60–69). Admittedly, we may be guilty of overinterpretation here, but it does occur to us that the act of tearing off Apsu's tiara is comparable to an act of castration. By so doing, Ea clearly renders his forebear powerless to resist, just as Kronos, Kumarbi et al. render their progenitors powerless by dismembering or 'biting' them. In any event, thus passes the first generation.

After having become 'king in heaven' Ea takes up residence upon the dead Apsu[29] and is joined by his wife Damkina. In time Damkina gives birth to the great Babylonian divinity Marduk, who is patently the Zeus-figure in this version. Many lines follow describing Marduk's brilliance and prowess, e.g. 'He was the loftiest of the gods, surpassing was his stature, /His members were enormous, he was exceeding tall' (i.97–100). Meanwhile, Tiamat schemes in order to seek revenge for the slaying of her husband. She causes a heretofore-unmentioned god, Kingu, to be elevated to the kingship, to command of the Assembly of the Gods—apparently, though the text makes no mention of it, displacing Ea. She then sets about creating a host of monsters (cf. i126). From this point on Ea fades into obscurity, and the threat presented by Tiamat and Kingu is met by Marduk. A great fight ensues. Marduk eventually slays Tiamat in single combat and destroys her host. He then captures the rebel Kingu and consigns him to Uggae, the god of the dead (i.v119–20). Afterward, Marduk and Ea[30] create heaven and earth from Tiamat's inflated body by splitting it in two (Kramer 1961:212; cf. the fate of Ymir). Marduk now reigns perpetually as 'king in heaven'.[31] He has validated his claim to sovereignty by emerging victorious in the epic duel with Tiamat and has banished the previous claimant to the title (i.e. Kingu).

Taken as a whole, the *Enūma Elish* presents some remarkable parallels to

the other versions of the 'kingship in heaven', though there are, of course, several important structural differences. For one thing, there is a bifurcation of the Kronos-figure. Taken together, the careers of Ea *and* Kingu approximate quite closely to that of the typical second-generation 'king': initially, in the person of Ea, we see him usurping the kingship through a ruse and (perhaps) performing an act of emasculation; later on, in the person of Kingu, we see him defeated by the Zeus-figure and consigned to what would appear to be a Tartaros-like place. In the case of Tiamat we have not bifurcation but fusion. She is at once Gaia and Typhon (cf. Zohak, who is both Kronos and Typhon). It is in the latter role, however, that she appears most clearly, and the fight between her and Marduk is strikingly similar to that between Zeus and Typhon, Teshub and Ullikummi, etc. The relationship between Tiamat and Kingu is, of course, the opposite of that between Kronos and Typhon or Kumarbi and Ullikummi: in the Babylonian account the 'king' is the creature of the monster. Yet on balance, it seems quite clear that the theogony contained in the *Enūma Elish* is akin to those we have previously surveyed. Indeed, the extent to which it presents parallels to the previously discussed Hittite-Hurrian version, coupled with its apparently unique position within the general framework of Babylonian mythology, led Speiser (1942) to suggest that the extant form of the text may well reflect what he terms 'an intrusive Hurro-Hittite myth.' Although he does make a good case for an immediate Hurro-Hittite influence, Speiser nevertheless goes on to suggest that the prototype of this creation myth is rooted deeply in the Sumero-Akkadian tradition and that it was from this tradition that it initially diffused to the Hurrians and Hittites (cf. Steiner, 1958:102–4). The correctness of this suggestion would appear to be borne out by the presence of the 'Theogony of Dunnu.'

Until recently, the *Enūma Elish* was the only Babylonian creation myth that came anywhere near to approximating the idea of the 'kingship in heaven.' However, in 1965 W. G. Lambert published (with A. R. Millard) and subsequently translated (Lambert and Walcot 1965)[32] a cuneiform text (BM 74329) which we may for convenience label the 'Theogony of Dunnu'. In it we can see essentially the same course of events as described in the *Enūma Elish*, although the locale and figures involved are for the most part quite distinct. The date of this new text is late. Lambert assigns it to the Late Babylonian period (i.e. between 635 and 330 B.C.), though it is suggested that it belongs to the earlier phase of this period and that, as in the case of the *Enūma Elish*, it would appear to contain materials originally composed perhaps as early as the beginning of the second millennium B.C.

Here, the action centers around the ancient city of Dunnu, an otherwise obscure place as far as the overall Babylonian tradition is concerned. The text itself is far shorter and more literary than the one just discussed. In this instance the two autochthons are the figures Ḫain (unknown outside of this text)[33] and Earth. Although the first three lines are incomplete, it appears that they give birth first by means of a plow to Sea (cf. Tiamat) and then in an apparently more normal fashion to the male figure Amakandu

(once again we encounter the ultimogeniture pattern). Amakandu is seduced by his mother: '[Earth] cast her eyes on Amakandu, her son, "Come, let me make love to you" she said to him'. (lines 8–9). After marrying his mother, Amakandu slays Ḫain and lays him to rest in the city of Dunnu, 'which he loved' (line 12). He then assumes his father's overlordship of the city (line 13). Subsequently, Amakandu marries Sea (his sister, cf. Kronos and Rhea), who gives birth to a son named Laḫar. Laḫar in turn slays his father, marries his mother (i.e. Sea), and assumes the kingship. He, too, produces offspring: a daughter, River, and a son whose name is unreadable. The latter eventually kills both Laḫar and Sea (his mother), marries River (his sister), and assumes power. At this point, the text is somewhat obliterated, but it appears that the pattern is carried on for at least two more generations. Sons slay their mothers and fathers, marry their sisters, usurp the sovereignty. The female figures here whose names are readable include Ga'um and Ningeštinna. There appears to be no clear termination of the text, which is contained on but the obverse and reverse of a single tablet, although the last readable line (40) would seem to indicate that the seat of power has been transferred to the city of Shupat(–) or Kupat(–).

In this 'Theogony of Dunnu' there can be seen in abbreviated and somewhat redundant form, the essential outlines of the theme in question. The fact that there are five generations rather than three is not, we feel, a crucial deterrant to putting the text into comparison with the other versions. It will be recalled that both the Hittite-Hurrian version and that of the Phoenicians, to say nothing of the version contained in the *Shāhnāmeh*, all describe one or more generations as existing prior to those upon which attention is centered. In the present case it would appear that the replication follows rather than precedes the principal sequence of events. To be sure there is a major structural problem here in that in all other versions surveyed, including the *Enūma Elish*, there is a denouement. One figure, be he Zeus, Teshub, Ba'al, Feridun, Odinn, or Marduk, puts a seal, as it were, on the succession. In the 'Theogony of Dunnu', however, Laḫar is in turn overthrown, and the series of usurpations is seemingly without end. Nor do we see here any indication of emasculation, although Walcot (p. 72), in comparing the text to Hesiod, suggests, perhaps leaning a bit too heavily upon Freud, that mother lust is equivalent to castration. On this point we suggest that the very brevity and laconic nature of the text perhaps precluded the elaboration of such details as how the sovereignty was transferred (e.g. via emasculation or the 'tearing off' of a tiara).

One interesting point of comparison between the Dunnu text and Hesiod's *Theogony* is the order in which Sea and River appear. According to Hesiod, the two figures associated with the sea, Pontos and Okeanos, emerge in succeeding generations. Pontos is essentially an autochthon, having emerged from Gaia without benefit of sexual intercourse, while Okeanos is born of Ouranos and Gaia. There is no doubt that Pontos is indeed the sea proper, but Okeanos is more specifically defined as the 'father of the rivers', the river that circles the earth (cf. *Theog.* 695–6). This order is paralleled

in the 'Theogony of Dunnu'; Sea is an autochthon, whereas River belongs to the third generation. Walcot (p. 72) also points out that there is a parallel to Hesiod here in the marital relationships that obtain. The Titans regularly contract sibling marriages (cf. Kronos and Rhea), 'but it is only Ouranos and Pontos who practice incest to the extent of mating with their own mother . . .' Again, in the 'Theogony of Dunnu' both Earth and Sea are involved in such marriages, and matings between siblings abound.

These, then, are the Babylonian versions. So far, no Sumerian counterpart has come to light, though several important Sumerian divinities are indeed present in the texts just discussed, e.g. Enki (Ea) and Anu. We do not mean to suggest that the Greek and other non-Babylonian versions of the 'kingship in heaven' are necessarily based specifically upon the *Enūma Elish* or the 'Theogony of Dunnu'. Rather, it is suggested that in one way or another all of the previously discussed theogonies[34] are based upon the immediate sources of these two Babylonian texts, sources which themselves would appear to have been Babylonian and perhaps even Sumerian. This suggestion is strengthened in that we now have two distinct versions of the theme, each dating from the early second millennium B.C., from two distinct centers of Babylonian culture, i.e. Babylon proper and Dunnu. The fact that one of these was a minor center adds even more weight.

Conclusions

As most of the principal points of comparison have already been noted and are summed up in Table I, it remains for us to return to the question posed by the title of this paper: Is the 'kingship in heaven' theme Indo-European?

Wikander's assumption (see above) that the theme is part of the Indo-European mythological inheritance is based principally upon the concordances between the Greek, Hittite, and Iranian versions. These concordances are indeed present and there is no doubt that the three versions are part of a single tradition. But the fact that these three Indo-European speaking communities share a common theogonic theme does not in itself mean that such a theme is part of their common mythological inheritance. By the same token, it can be said that a fair number of Indo-European speaking communities today share a common belief in how the world was created: i.e. by a God whose principal attributes would appear to be omnipotence and an intense jealousy of all other pretenders to divine status. There is, of course, no doubt whatsoever that this idea was borrowed from the Judaic tradition. We have a clear record of when and where the borrowing occurred. But suppose we did not have such a record; suppose, say, that a Martian scholar eons hence were to be confronted with this common tradition—present, albeit, among non-Indo-European speakers as well, but let us assume for the purposes of argument that the dates and times of these borrowings were indeed known. Would he not be tempted to view these relationships from a genetic standpoint? Would he not be tempted to reconstruct a common Indo-European cosmogony involving 'the hand of God moving across the waters,' etc.?

We suggest that those who argue for an Indo-European origin as far as the 'kingship in heaven' is concerned have most likely fallen into the same trap as our hypothetical Martian. The problem here is that, unlike the spread of the Judaic cosmogony, the spread of the idea of the divine succession cannot be documented. There is no clear record of events to mark its spread from ancient Babylonia to Anatolia, Phoenicia, Iran, Greece, and ultimately, perhaps, to Scandinavia. There is nothing here comparable to the conversion of Clovis or the ministry of St. Patrick. Yet spread it did, from one religious system to another, the initial impetus being perhaps the prestige of the Babylonian tradition.[35] By the time the theme reached Scandinavia—if indeed it did not evolve there independently (see above)—the Babylonian roots had long since become obscure, and the prestige would have been that of the Greco-Roman tradition, to which it had diffused perhaps two millennia earlier.

Two of the chief reasons, it seems to us, for ruling out an Indo-European origin is the absence, previously noted, of the theme in the Indic tradition and its very late appearance in that of ancient Iran. Also important in this connection is its absence in the ancient Celtic tradition. Admittedly, much of our knowledge of Celtic religion is confined to those elements of it that persisted in Ireland; yet even here, given the care taken by the medieval Irish monks to preserve their heritage, one would assume that if the theme had been present before the arrival of Christianity, it would have been carried over. But only by the most procrustian of methods can one make a case for its presence in the traditions relating to the Tuatha de Danann and their predecessors. It is simply not present.

Thus, that neither the *Vedas*, the *Avesta*, nor the *Lebor Gabala* know the theme is highly significant. With the exception of the Norse, all known versions center upon Mesopotamia. Even Egypt does not appear to have known a clear version of the 'kingship in heaven.'[36] Its diffusion to Greece, usually thought to have been accomplished via Phoenicia (cf. Güterbock 1948:133–4), may indeed have been much earlier than heretofore suspected. Lambert and Walcot (1965:72) suggest that the mythical conception of a divine kingship may have diffused directly from Mesopotamia to Greece during the Late Helladic period and cite recent archeological evidence (e.g. the presence of Babylonian cylinder seals at Thebes) indicative of widespread Mycenean-Mesopotamian contacts.[37] Its diffusion to the Hittites would probably have been accomplished via the Hurrians, who had come into close contact with the mainstream of Mesopotamian civilization by the end of the third millennium B.C. Its late diffusion eastward to Iran may reflect more immediately the Hesiodic and/or Phoenician versions rather than the Babylonian versions themselves (cf. the very specific parallels between Zohak and Typhon). But whatever the course taken, its absence in at least three widely separated and important Indo-European traditions, coupled with the early dates of the Babylonian versions, would seem to give powerful support to our contention that it is indeed of Babylonian and perhaps even ultimately of Sumerian origin.

No discussion of Indo-European origins would, of course, be complete

without reference to Professor Dumézil's theory of a common tripartite social and supernatural system, a system which is clearly evident throughout the ancient Indo-European speaking domain.[38] Dumézil himself has refrained from any attempt to apply the tripartite model to the idea of the 'kingship in heaven', despite the fact that at first glance, at least, the three-generational sequence would seem to offer a fertile field in this regard. Even Wikander, in many ways Dumézil's most brilliant disciple, and, as we have seen, the chief proponent of the Indo-European origin theory, has refrained from attempting such an application (Littleton, 1966:146–202). The closest Dumézil himself has come to the problem is the suggestion (1959:263–83) that the proto-Indo-European mythology possibly included a tradition that Heaven was the last and only surviving offspring of a great water deity (probably female), who drowned all but one of her children as soon as they were born. His suggestion is based upon a comparison between the Norse god Heimdallr and the Vedic divinity Dyauh. Both are connected with water. Heimdallr is said to have had nine brothers, who were conceived as 'sea waves'; Dyauh and his seven brothers (the *Vasus*) are linked with the river goddess Ganga (Ganges), who drowns seven of the eight siblings, leaving only Dyauh to reach maturity (cf. *Mahābhārata* 1.3843–963).

Certainly, the personifications of sea and heaven play parts in both the Greek and the Babylonian versions of the kingship in heaven (cf. Pontos, Tiamat), but in neither case do we have any clear identification of the drowning of all but one of their offspring. Moreover, Heimdallr plays no part whatsoever in the Norse version, nor do any of the other traditions surrounding the rather otiose Dyauh come anywhere near to approximating the sort of divine succession so crucial to the presence of the theme in question. In short, it would seem that, even though Dumézil may well be correct in his suggestions as to the Indo-European roots of the traditions surrounding the births of Dyauh and Heimdallr, it would not be germane to the problem at hand. The presence of such a common Indo-European tradition would in no way alter our convictions as to the origin of the theme.

Although he did not consider the theme as a whole, some years ago Vian (1960) attempted an interpretation of the Typhon episode in Dumézilian terms. As Vian saw it, Zeus' battle with the monster derives from an isolated story paralleling what Dumézil (1942) suggests is the typical Indo-European myth of a fight between a 'second function' or warrior figure (in some instances a triad of them) and a tricephalic monster (cf. the Indian account of the conflict between Indra and the tricephalic son of Tvastar, the Roman tradition of a fight between the three Horatii and the three Curiatii). This story later fused with a widespread non-Indo-European dragon-slaying account (which had also diffused to the Hittites; cf. the Ullikummi and Illuyanka narratives)[39] that had been introduced into Greece by the Phoenicians in the early eighth century B.C. Thus, according to Vian, by Hesiod's time the story as it is generally known in Greek mythology had fairly well crystallized. It is interesting that in later versions

TABLE I

NOTE: the italicized names are those of the principal figures involved in the 'kingship in heaven'

Version	1st Generation Figure	2nd Generation Figure	3rd Generation Figure	Monster
Greek	*Ouranos:* autochthon; marries Earth (Gaia); exiles offspring; gives birth to	*Kronos:* castrates and exiles father; swallows offspring; marries sister (Rhea); gives birth to	*Zeus:* deposes father; must validate his position by slaying	*Typhon:* creature (or offspring) of *Kronos*; is killed in single combat by *Zeus* around Mt. Kasios.
Hurrian-Hittite	*Anu:* son of Alalu; deposes him; gives birth (?) to	*Kumarbi:* castrates and deposes father; swallows offspring (?); gives birth to	*Teshub:* deposes *Kumarbi*; must validate his position by slaying	*Ullikummi;* creature of Kumarbi killed by Teshub *et al.*, near Mt. Ḫazzi (Kasios)
Phoenician	*Ouranos:* offspring of Elium; gives birth to	*El* (Kronos): castrates and deposes father; marries three sisters (daughters of *Ouranos*) gives birth to	*Ba'al:* deposes father; causes latter to castrate self; reigns perpetually.	(Typhon is mentioned, but no clear monster figure.)
Iranian	*Jamshid:* preceded by three earlier kings; is deposed by	*Zohak:* dragonlike figure; cuts *Jamshid* in two; marries two sisters (of *Jamshid*) is deposed by	*Feridun:* kinsman of *Jamshid*; kills *Zohak* in single combat; reigns triumphantly.	*Zohak:* clearly parallels Typhon; fusion of 2nd generation and monster.
Norse	*Two lines:* (1) *Buri:* autochthon; gives birth to Bör; (2) *Ymir:* giant autochthon; is licked from ice by cow; gives birth to *Thrudhgelmir.*	(1) Bör: gives birth to *Odinn*; (2) *Thrudhgelmir* gives birth to *Bergelmir.*	(1) *Odinn:* kills and cuts up *Ymir*; reigns perpetually; (2) *Bergelmir:* is banished by *Odinn* to Jöntinheim.	*Ymir:* from whose corpse the universe is made; fusion of 1st generation figure and monster.
Babylonian				
(1) *Enūma Elish*	*Apsu:* Autochthon; marries *Tiamat*; threatens offspring; gives birth to	*Ea:* deposes father; position usurped by *Kingu*, a creature of *Tiamat*; gives birth to	*Marduk:* deposes *Kingu*; reigns perpetually; must validate position by slaying	*Tiamat:* monstrous figure; her corpse is used to make universe; fusion of 1st generation figure and monster.
(2) "Theogony of Dunnu"	*Ḫain:* autochthon; marries Earth; gives birth to	*Amakandu:* slays father, marries mother, then Sea; gives birth to	*Lahar:* slays father; marries mother; is eventually deposed by offspring; not perpetual ruler.	(no clear monster figure)

of the Typhon episode (i.e. those of Apollodorus and Nonnos) the details undergo progressive elaboration and become more and more similar to those of the Hurrian-Hittite version. As has been noted, the description of Ullikummi before Mount Hazzi is almost identical to Nonnos' description of Typhon.

If Vian is correct, then some intriguing albeit remote possibilities present themselves. Perhaps the Hittites, too, grafted the dragon-slaying myth

onto an inherited Indo-European three-headed monster tale and, like their Greek cousins a thousand years later, eventually fused this with the Babylonian account of the divine kingship. There would thus be three distinct strata in both the Greek and Hittite versions: (1) the Indo-European account of the slaying of the tricephalus, (2) the dragon-slaying myth, and (3) the 'kingship in heaven' theme proper. Of course, it is by no means clear that the monster-slaying episode is disassociated in its origins from the rest of the theme under discussion. It is just as easy to make a case for the presence of this figure in the person of Tiamat—though its presence in the 'Theogony of Dunnu' is less clear.

In sum, while it presents some alternative possibilities of interpretation of certain specific episodes associated with the 'kingship in heaven,' we do not feel that Vian's arguments relative to Typhon add anything substantive to the argument favoring an Indo-European origin for the theme as a whole, and we must reiterate our conclusion that it is most probably rooted in the Babylonian tradition. The search for a common Indo-European theogony must continue.

Notes

[1] In this usage we follow Güterbock (1961:155).

[2] Although he was unable to be present when the first draft of this paper was read in Philadelphia (April 21, 1966), Professor Hans G. Güterbock was kind enough to read it most carefully at a later date and to offer many valuable comments and suggestions. It goes without saying that I am deeply in his debt. I should also like to thank the other participants in the Conference, especially Professor Jaan Puhvel, for their equally welcome comments and suggestions, many of which have been incorporated in the present version.

[3] A more comprehensive treatment of the evidence relative to the kingship in heaven, especially the Greek, Phoenician, and Hittite-Hurrian evidence, is contained in Littleton (in press).

[4] Professor Güterbock has called my attention to a doctoral dissertation by Gerd Steiner (1958), not as yet published, in which the author includes the *Enūma Elish* among the four 'Quellen' of the theme although he does not consider the relevant Norse and Iranian traditions.

[5] By *theme* we mean an expression of an idea or set of ideas rather than a specific narrative pattern. Otherwise, our use of the term theme here comes close to approximating Thompson's (1946:415) definition of tale-type: 'a traditional tale that has an independent existence.'

[6] According to Apollodorus (*Bib.* I.3.2–3), Zeus uses a sickle to inflict a mortal wound upon Typhon, who flees to Mount Kasios. There, however, the monster is able to wrest the blade from Zeus and use it to sever the sinews of the latter's hands and feet. After performing this multilation Typhon's power returns and he carries the incapacitated Zeus to the famous Corycian cave, in Cilicia. Hiding the sinews, he leaves the she-dragon Delphyne to guard his prisoner. However, Hermes and Aigipan steal the sinews from their bearskin hiding place and, unobserved by the monster, fit them again to Zeus, thus enabling him to continue the fight. There is also a tradition (cf. Oppian, *Halientica* 3.15–25) wherein Corycian Pan lures Typhon out of the cave with a fish meal so that Zeus' bolts can kill him on the seashore.

[7] Known only through the works of Eusebius (*Praeparatio Evangelica*) and Porphyrius (*De Abstinentia*). Philo claims to have gotten his information from the works of a certain Sanchunjathon who, it is asserted, lived 'before the Trojan War.'

[8] A further argument against interpreting Philo wholly in syncretistic terms is that

he, like the Hurrian-Hittite texts shortly to be considered, preserves a 'pre-Ouranos' figure, i.e. Eliun. Cf. the position of Alalu in the Hurrian-Hittite version.

[9] The fact that a god Alala is listed in a Babylonian god list as a father of Anu leaves no doubt as to the filial relationship here (Güterbock 1961:160).

[10] Although the Hittite reading of the ideogram for Weather-God is as yet unknown it is highly probable that, given this and other contexts (cf. the Ullikummi texts), the Hittite deity in question here can be none other than the Hurrian Weather- (or Storm-) God Teshub (cf. Güterbock 1946:35, 1948:124, 1961:158; Goetze 1955:120).

[11] From a reference in col. ii (see note 12 below) it appears that these latter two divinities are Marduk (represented by a rare Sumerian name) and one whose name is written with the word sign KA.ZAL 'lust' (Güterbock 1961:158).

[12] At first Kumarbi attempts to avoid bearing these unwelcome offspring by spitting out Anu's seed (end of col. i); nevertheless a fragmentary reference to Kumarbi's failure to count months and the phrase 'the ninth month came' clearly indicates that he carried Teshub et al. within him for a full term. Güterbock (1961: 157–8) points out that the theme of the mutilated first part of col. ii is childbirth and that two of the gods in Kumarbi's 'interior', Marduk and KA.ZAL (see note 11 above), discuss with him several ways in which they might be born.

[13] In col. iii.19–22 there is the suggestion that Anu wants to make Ea king instead of Teshub (cf. Güterbock 1961:159; Meriggi 1953:125), which would perhaps reflect the *Enūma Elish* (see below).

[14] Literally, the 'first man.'

[15] Atkinson (1832:140) asserts that Jamshid was laid between two planks and sawed lengthwise.

[16] Or imprisons him in a cave; cf. Wikander (1951:47).

[17] The Warners (1905:171–4) suggest that Feridun is actually a coalescense of two Avestan figures, Thraetaona and Thrita (cf. *Yasna* 9.21–30).

[18] For a discussion of the possible Vedic parallels here, see Wikander (1951:46).

[19] For a discussion of the extent to which Snorri, to say nothing of Saxo Grammaticus, was influenced by the *Aeneid*, see Littleton (1965:24–5).

[20] The translations utilized here of Snorri's and Saemundar's *Eddas* are, respectively, Young (1954) and Hollander (1926).

[21] In the Gylfaginning Snorri describes Bergelmir's flight as occuring in the context of a universal flood created from the blood which gushed from Ymir after his demise. As this is the only clear reference to a flood in Norse myth, it seems reasonable to infer that Snorri, as a Christian, felt the need of it. This is underscored by the etymology of the word *lúðr*, used by him to refer to the 'boat' in which Bergelmir and his wife survive the deluge. Although Snorri clearly uses the word in the context of 'boat' earlier usages (cf. the *Vafthrudhnismal* 35) would seem to indicate that it meant 'coffin' or 'bier.' For further discussion of this word, see Turville-Petre (1960: 211–12); Petersson (1909:267–9), who asserts that the basic meaning is 'hollowed tree-trunk' and proposes a derivation from Indo-European **lú-tró-* (cf. Skt. *lunáti* 'cut, clip').

[22] We are not the first to make such an observation. Jakob Grimm (1883, II:575), had occasion to observe 'As the Edda has a Buri and a Börr before Odinn, so do Uranus and Kronus here come before Zeus; with Zeus and Odinn begins the race of gods proper, and Poseidon and Hades complete the fraternal trio, like Vili and Ve. The enmity of gods and titans is therefore that of ases and giants . . . .'

[23] Walcot (1966:33) suggests that the rise of Marduk to supremacy among the gods of Mesopotamia was quite late and that the *Enūma Elish* as we know it was most likely composed around 1100 B.C. He thus concludes that 'In terms of chronology, Enuma Elish now seems to stand between the Hattusas tablets and the Theogony . . . .' This, however, would not preclude earlier prototypes wherein some other god played the part of Marduk.

[24] Cf. Sumerian Abzu (Kramer 1961:120).

[25] I.e., 'Mother' Tiamat (Speiser 1955:61).

[26] The translation of the *Enūma Elish* utilized here is that of Speiser (1955).

[27] Cf. i.52, wherein Apsu plots 'against the gods, his sons'; i.56, wherein Apsu's intentions are made known 'unto the gods, their first-born.'

[28] Not to be confused with the epithet of Tiamat (see note 24 above).

[29] There is some confusion here. Kramer (1961:121) suggests that Ea actually takes up residence upon Apsu's corpse. However, the text itself would seem to indicate that Ea names his place of residence (i.e. the location of the 'cult hut'; cf. 1.177) *after* his deceased parent.

[30] West (1966:23) asserts that Ea's elder sibling (or grandfather) Anshar assumes the kingship. Nowhere in the text is this clearly evident. The only passage that may possibly reflect such a royal status is iii.lff., wherein Anshar sends a message to Lahmu and Lahamu via '. . . Gaga, his *vizir*' (italics mine). Otherwise, Anshar appears as but one of the siblings (or forebears) of Ea who plays a prominant albeit essentially supporting role in the deposition of Apsu and the subsequent conflict with Tiamat.

[31] I.e. 'He took from him [Kingu] the Tablets of Fate, not rightfully his,/Sealed (them) with a seal and fastened (them) on his breast' (iv.121–2).

[32] It should be pointed out that Walcot is responsible for the appended 'classical commentary' (pp. 68–72); the translation and accompanying commentary is that of Lambert.

[33] Lambert (1965:66–67) suggests that the two signs *ḫa-in* may have been miscopied from the one large sign used to write the name of the corn goddess Nidaba.

[34] The Norse version would appear to be only indirectly derived from that which took shape in Babylonia some 4000 years ago; its immediate roots (if it is not wholly independent) probably lay in the version best known to the Greco-Roman world, i.e. that of Hesiod et al.

[35] The historical relationships proposed here are generally congruent with those proposed by Steiner (1958:104), who sees the *Enūma Elish* as the immediate source of two intermediate albeit unattested versions labeled 'X' and 'Y.' The 'Y' version, Steiner suggests, ultimately reached Greece and manifested itself in Hesiod's *Theogony*, etc. The Hurrian-Hittite version reflects both the 'X' and the 'Y' versions, although the former would seem to be the most immediate source. He also suggests that the 'X' version perhaps gave rise to a third unattested version, 'Z', which, together with the Hesiodic version, is reflected in Philo's 'history.' If Steiner is correct it might be suggested that his hypothetical 'Z' version could have diffused to Iran as well as to Phoenicia (cf. the specific correspondences between Philo and Firdausi as noted in Table I and elsewhere); The Hesiodic version would be the immediate source of that contained in Snorri's *Edda*.

[36] However, Walcot (1965:69) points out that there is an Egyptian tradition according to which sky devoured her children, and quarrelled with her husband, Earth, and therefore they were separated.

[37] Walcot (1966:47) has since modified his views and is convinced that the theme most likely diffused to Greece only after sustained contact had been reestablished with the Near East in the eighth century B.C.

[38] The most succinct statement of Dumézil's theory can be found in his *L'idéologie tripartie des Indo-Européens* (1958). For an analysis of this theory, see Littleton (1964, 1966).

[39] For a thorough discussion of the dragon-slaying myth, see Fontenrose (1959).

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An Indo-European Mythological Theme in Germanic Tradition

Donald J. Ward

Over a century ago scholars investigating the mythologies and religions of various European and Asian peoples discovered a recurring pattern of twin divinities among the pantheons of peoples who spoke languages related to the Indo-European proto-language, and they concluded that the Divine Twins were important members of the pantheon of the Proto-Indo-Europeans.[1] This evidence is of such a nature that it remains one of the most striking single justifications for the comparative study of Indo-European mythology.

The Indo-European tradition of the twins is best preserved in the hymns of the Rigveda, in Greek mythology, and in the folk songs of the Baltic area.[2] In all three traditions the twin gods are known as the sons of a divinity of the sky. In the Vedic hymns the twins are the Aśvins, sons of Dyaus 'the Sky' and are accordingly called *Divó nápātā* 'Sons of God' (RV 1.117.12). This name corresponds precisely to the Greek *Diòs koûroi*, who are Kastor and Polydeukes, sons of Zeus, who is likewise a sky-god. The identical name is preserved in Baltic mythology, where the twins are called *Dieva dēli* in Latvian and *Dievo suneliai* in Lithuanian, both of which mean 'Sons of God'. The fathers of the twins are not only related mythologically, but etymologically as well, for their names can be traced to a common Indo-European source.[3]

In all three traditions the Divine Twins have a sister. In the Rigveda she is Sūryā, whose name is the feminine equivalent of *Sūrya* 'the Sun'. She also bears the name *Duhitā Sūryasya* 'Daughter of the Sun' (RV 1.116.17). This name finds a precise parallel in the name of the sister of the Latvian twins, *Saules meita* 'Daughter of the Sun', who in the Lithuanian songs is called *Saules dukterys*, likewise 'Daughter of the Sun'. In Greek mythology the sister of the Dioskouroi is Helen, whose very name means approximately 'divine splendor', and who, according to some reports, is likewise known as the daughter of *Hḗlios* 'the Sun' (Ptolem., *Hist. Nov.* 189; Phot., *Bibl.* 149a).

There is a polyandrous, incestuous element present in the Vedic hymns, for the Aśvins are not only the brothers of Sūryā, but her suitors (RV 10, 85, 9), and even her joint husbands (RV 4.43.6; 1.119.5). Similarly, the Latvian songs report that the *Dieva dēli* are the suitors of the Daughter of the Sun. In the Vedic hymns, Sūryā mounts the chariot of the twins (RV 1,

117, 13) and she is seen orbiting the heavens with them (RV 4.43.2–3). Similarly, according to Euripides (*Hel.*, 1495) the Dioskouroi lead Helen home in a horse-drawn chariot. In the Baltic tradition the Sons of God take the Daughter of the Sun sailing in a boat at sea.[4]

In each of the Indo-European traditions, the association of the Divine Twins with the horse represents the most pronounced characteristic of the twin brethren. In the Rigveda the very name of the divine pair expresses this relationship: *Aśvínā* 'owners of horses'. Similarly, the Greek Dioskouroi were called *leukópōloi* 'having white horses' (Pindar. Pyth. 1, 66). They were also referred to as *hippótai sophoí* 'skilled riders' (Alkman, *Fragment* 12). This association of the twins with the horse also occurs in Baltic mythology. The horses of the twin deities are seen outside the Sun Maiden's door (363–33801), or on the hillside eating oats (363–33769). Moreover, three golden steeds are seen accompanying the sun across the sky (394–54926). The third horse may indicate a theriomorphic representation of the Sun Maiden.

There are countless other traits and functions which the various pairs of divinities share. They are saviors at sea, they are associated with stars, they are divinities of fertility and of abundance, they are divinities of warfare, they are magic healers, and they are associated with the swan.[5] The similarity of each of these traits and functions is so striking in detail that there can be no question that each of them belonged to a stratum of mythology during a period of Indo-European unity.

Inasmuch as it has long been established that the traditions involving the Divine Twins date back to Indo-European times, one would expect to encounter the twins in Germanic mythology. There have been countless investigations undertaken in the attempt to establish evidence of the Germanic Divine Twins, and although some of them have produced fruitful results, the fact remains that nowhere in Germanic mythology do we have unambiguous evidence comparable to that which is available for the Greek, Baltic and Vedic traditions. In a recent investigation, Karl Schneider has posited the figures of Balder and Freyr as the Germanic Dioscuri.[6] Schneider bases his contention on the evidence which demonstrates that the traits and functions of the two gods are identical with those of other Indo-European Divine Twins. Although this is undoubtedly true, this fact alone does not permit one to assume that the two divinities represent a Dioscuric pair.

A very fruitful, if not entirely convincing, attempt to locate the twins among Norse deities was made by Magnus Olsen.[7] By investigating place names reflecting the names of divinities, the author discovered an evident association between the gods Ullr and Freyr. Assuming these locations represented cult sites, Olsen has shown that in a number of instances the two deities had sites neighboring on one another, thus indicating that they were worshipped as a pair.

Among the Norse pairs most frequently posited as the Divine Twins are *Balder* and *Hödr*, and one of the more convincing arguments for this hypothesis was put forward by Viktor Rydberg,[8] who saw in Saxo's treatment

the form which best represented the Dioscuric tradition. In this euhemerized treatment, the two brothers become rivals for the hand of Nanna, who is supposed to represent the third member of a Dioscuric triad. However convincing Rydberg's study may be, it must nevertheless be stressed that traditions involving the rivalry of a pair of brothers over one girl are so widespread that one can scarcely use such a story as evidence of an Indo-European mythological theme.

Georges Dumézil has convincingly demonstrated that the Vanic divinities, Njördr, Freyr, and Freya, represented an Indo-European Dioscuric triad.[9] As divinities of fertility and abundance ('third function'), they had difficulty gaining acceptance into the realm of the Aesir. Dumézil shows a striking parallel with the Vedic Aśvins, who likewise as gods of fertility and of herdsmen and masses were not permitted to join in the Soma sacrifice with the other gods. The difficulty most apparent with this theory is that Njördr is generally considered the father of Freyr and Freya and not their brother. However, as Dumézil points out, there is evidence which indicates that Njördr and Freyr were indeed once thought of as a pair. Elias Wessén, for example, has shown where a Skaldic poet has used both names with the singular form of the verb 'to have' (*hefr*).[10] Dumézil also points out that the Norse Ynglingar considered themselves descendants of Njördr and Freyr. A thorough search through the source material reveals a number of similar instances of Njördr and Freyr either appearing together, or being invoked as a pair. For example, when Sigurðr Hlaðajarl conducted the great offering in Trondheim (*Hreimskringla Saga Hákomar góða* 14), the first drink was consecrated to Odinn, and the following to Njördr and Freyr. Moreover, a Skaldic verse from chapter LVI of *Egils Saga Skallagrímssonar* invokes Njördr and Freyr together to persecute those guilty of wrongdoings. Further, an Icelandic oath (*Ulfjutrecht* 933) invokes the two gods as witnesses.[11] Further evidence of the Dioscuric nature of these two gods may well be indicated by the occurrence of the name Njördr in the plural: *hjǫr-Niðir* 'Njördrs of the sword.'[12] This plural form could represent an important instance of *devatādvanda* in elliptic form, analogous to Vedic *Mitrā́* for Mitra and Varuṇa, and to the Latin *Castores* for Castor and Pollux. It is such internal evidence which speaks most strongly in favor of identifying the pair Njördr and Freyr with the Indo-European tradition of Divine Twins.[13]

Hans Naumann has recently devoted a study to the Divine Twins[14] and has concluded that the Germanic pantheon had various Dioscuric pairs. Among them were Vili and Vé. The other pairs are those who are to rule in the new age after *Ragnarök*: Baldr-Hödr; Vidar-Vali; Modi-Magni; and the two unnamed sons of Vili and Vé.

Although much convincing evidence has been adduced in support of the Dioscuric nature of the above pairs of divinities, the very fact that the pantheon offers so many such pairs is an indication that there is not a clearly recognizable Dioscuric pair in the material which has been preserved. Yet one cannot doubt that the Germanic peoples once knew the Divine Twins. Tacitus (*Germania*, 43) reports that a group of people known

as the *Naharvali* performed a ceremony in honor of a pair of gods known as the *Alcis* who were, according to Tacitus, 'Castor and Pollux according to Roman interpretation.' The ritual was conducted by a priest who was 'adorned as a female.' The interpretations of the name 'Alcis' are many and no attempt will be made to state them all here.[15] Suffice it to say that the evidence in *Germania* makes it apparent that a Dioscuric pair was worshipped at one time by at least one Germanic tribe.

There evidently have been euhemerized manifestations of the mythological twins in Germanic heroic traditions as documented during the migrations. Paulus Diaconus (*Hist. Langob.* I, 7) speaks of Ambr and Assi as the leaders of the Vandals and of Ibor and Aio, whose mother was called Gambara, as the leaders of the Langobards. This latter group occurs in Saxo (Book VIII) with the names Ebbo, Aggo, and Gambaruc. In both sources, the brothers, whose names have been associated with Dioscuric functions,[16] were leaders of migrations to establish new colonies. They were chosen for this role by the drawing of lots during periods of famine and overpopulation.

Dio Cassius (71.12) reports that in the year 171 A.D. a people known as the Asdingi appeared on the border of Dacia under the leadership of a pair of youths, Raos and Raptos, whose names mean approximately 'pole' and 'post'.[17] Since aniconic wooden pillars were associated with the Dioskouroi in Greece, these names are of considerable significance. It is not unlikely that such pillars were associated with the Divine Twins since Indo-European times, and that these two heroic figures represent the Germanic twin gods in euhemerized form.

In the 'Catalogue of Sea-Faring Kings' of Snorri's *Edda*, there occurs the names of a pair of heroic kings, Vinill and Vandill.[18] These names probably reflect a pair of Divine Twins from whom the Germanic tribes, the Vinniler and the Vandali, acquired their names. It was mentioned above that there have been reports of dual kings for each of these peoples. The Vinniler were led by Ibor and Aio, while the Vandali were led by Ambri and Assi. Such evidence could point to a long history of Dioscuric kingships, perhaps even extending over centuries. Indeed, there is evidence that the practice of a people taking its name from a Divine Twin continued even after new tribes migrated from the homeland. For example, the name Ambrones, which evidently comes from the Dioscuric name *Ambri* has been documented for the name of a tribe. Similarly, there is evidence of another people called *Assipitti*, which probably reflects the name Assi.[19]

Although reports for Germanic dual kings are generally rather scant there is an abundance of historical sources recording the exploits of the famous young brothers, Hengist and Horsa, who are reported to have led the Anglo-Saxon invasions of the British Isles. In comparison with the continent, the British histories are of a more recent date, none being older than the eighth century A.D. Nevertheless, there are some striking similarities between the Anglo-Saxon traditions and the traditions from the continent.

The names of the twins evidently both mean 'horse,' indicating that the same therimorphic concept associated with other Divine Twins of the Indo-

European tradition was known also among Germanic peoples. There is a probability that wooden idols were also associated with the worship of the pair, for in the village of Jevenstedt in Schleswig-Holstein, and in Hermannsburg in the Lunenburg heath, wooden horses adorning the gables of peasant houses were still called *Hengst* and *Hors* in the late nineteenth century.[20]

British chronicles and genealogies generally emphasize a divine origin for the pair. For example, Bede, Nennius, Aethelweard, William of Malmesbury, and many others trace the pair directly from *Woden* and *Freya*.[21]

Various English histories contain episodes involving the brethren which are evidently Dioscuric mythological legends in heroic garb. The chronicle of Aethelweard, for example, which dates from the end of the tenth century A.D., gives an account of the plight of King Vurthern of Britain who, being besieged on all sides by his enemies, sent messengers to the Saxons for help. Hengist and Horsa, two young men already renowned in Germany as the grandsons of Woden, suddenly appeared with three vessels loaded with arms. The two youths took charge of the attack and soon succeeded in routing the enemy. There can be little doubt that this report reflects a Dioscuric legend in which the twin brothers appear suddenly by sea to save the besieged armies. This is the precise theme encountered frequently in the mythologies of other Indo-European peoples.[22]

Another report which is evidently based on mythological legend tells that the Saxons, plagued by overpopulation and famine, drew lots for the purpose of choosing the members of an expedition to found a new colony. As a result of the drawing, Hengist and Horsa were chosen as the leaders.[23] This episode constitutes a precise parallel to the above-cited accounts of the migration of the Vinniler, whose leaders, Ibor and Aio, were likewise chosen during a period of overpopulation and famine by the drawing of lots. Since the two episodes are almost identical in detail, it appears evident that each is based on a Germanic Dioscuric legend. This assumption is supported by the fact that the Indo-European Dioscuri were frequently associated with the establishment of new settlements.[24]

In the history of Suffridus Petrus (*De Fris. Antiq. et Orig.* II, 15) we learn that the original Hengist and Horsa had a sister named *Swana* 'Swan.' It is possible that this girl represents the third member of the original Dioscuric triad, and thus is equivalent to the Sun Maiden of other Indo-European traditions. This hypothesis is supported by the evidence from other Indo-European traditions which links the Divine Twins to the Swan. For example a Vedic hymn indicates that swans were envisioned pulling the chariot of the Aśvins (RV 4.45.4). Moreover, there is a theriomorphic element involved, for the twins themselves were thought of as celestial birds (RV 4.43.3). In Greece it is reported that Zeus assumed the form of a swan when he approached Leda and begot the Dioskouroi. Moreover, it is reported that Kastor and Polydeukes were hatched from an egg, as was their sister, Helen.[25]

There is other evidence of the sister of the Divine Twins being associated

with the swan in Germanic tradition. The heroic brothers Sörli (Sarus) and Hamdir (Ammius) slay King Ermanaric and thus avenge their sister, Svanhild.[26] This episode has been interpreted previously as a heroic euhemerization of a Dioscuric myth.[27] The name is also mentioned in the *Foraldor saga* 27 where *Svanhild Gullfiödr* is reported to be the daughter of *Dagr* 'Day' and of *Sol* 'Sun'. Thus the evidence speaks overwhelmingly in favor of associating *Swana* with the Indo-European 'Daughter of the Sun'.

In the sources reporting the deeds of the Germanic dual kings, the Dioscuric legends had been euhemerized, that is, the celestial divinities had become mortal heroes, and their deeds had become heroic legends, or even 'historic' events. In view of this euhemerization, it is the heroic narrative to which one must turn if one is to locate additional evidence of the Divine Twins in Germanic tradition. Various epics, lays, and similar writings show unmistakable parallels with the Indo-European Dioscuric tradition. Among the more striking is the rescue episode of the Middle High German heroic epic, *Kudrun*.[28]

The rescue episode of this epic has long posed one of the more perplexing problems for the investigators interested in the prehistory of the epic. The scene in question occurs in the third and final section of the epic:

> The lovely princess Kudrun is promised in marriage to Herwig; however, before the wedding the maiden is abducted by Hartmut and taken to Ormanie (Normandy). Kudrun refuses to have anything to do with her abductor, and as a result of her obstinacy, she is turned over to the care of Hartmut's mother, Gerlind, who forces the princess to perform many difficult and humiliating tasks. One wintry morning in March, some thirteen years after her abduction, Kudrun is at the seashore washing clothes when she spots a boat at sea bearing two knights. They are Kudrun's fiancé, Herwig, and her brother, Ortwein, who have come to liberate the maiden. After a recognition scene, Kudrun casts the clothes into the water and returns to the castle. The following morning Herwig and Ortwein return, leading the large army. They defeat the Normans and liberate Kudrun.

The fact that two knights appear to rescue one maiden in distress has been the key point of contention among scholars, and it has generally been assumed that one of the pair must have been the sole and original rescuer in the source material used by the author of the epic.[29] This assumption has generally been based on the fact that there exists a group of popular ballads treating this theme in which there is only one rescuer, most often the brother. The relationship between the ballads and the epic is a complex problem which will not be treated here. It suffices to call attention to the study of the Hispanist, Ramón Menéndez Pidal.[30] By listing a large group of Kudrun ballads in which the original duality is preserved, and by demonstrating that in other ballads the husband, and not the brother, is the sole rescuer, Menéndez Pidal has shown that the duality must have been a primary element in the ballads. In some cases the *Zersingen* process reduced the number of rescuers to one; sometimes it was the husband, sometimes the

brother, who was eliminated. Menéndez Pidal's findings are very convincing and they are of importance for the study of the prehistory of the epic, for it has now become clear that the author of *Kudrun* did not use a ballad or any other source in which there was only one rescuer. The duality evidently belongs to the tradition from which the rescue episode stems. I suggest that it was an Indo-European Dioscuric theme which lies at the base of this part of the epic. In this euhemerized treatment the two shining knights, who suddenly appear in a boat at sea to rescue the princess at the seashore, represent the Divine Twins who have come to liberate their sister and betrothed, the Sun Maiden. Indeed, an investigation of the various sources reveals a mythological theme so identical in detail to the above rescue episode that a relationship is highly probable.

The most famous treatment of the theme is found in Greek tradition, namely in the story of the abduction of Helen by Theseus (Herod., *Hist.* IX, 73; Alcman, *Fragment* 12; Paus. I.41.4; Plutarch, 31ff.). Essentially, the story is as follows: When Helen was still a child, she was dancing in the temple of Artemis Orthia when she was carried off by the then fifty-year-old Theseus. Some men were sent in pursuit, but they followed no farther than Tegea. Because of her youth, the maiden was delivered by Theseus to his mother, Aethra, for care. Kastor and Polydeukes, learning of the abduction, proceeded to Athens where they demanded their sister. They learned that she was a captive of Aethra in Aphidnae. They stormed the city, defeated the defending armies, won the town, and successfully liberated their sister. They also took Aethra captive. All this occurred while Theseus was absent.

There are similar rescue episodes recorded involving other pairs of Greek twins. The Theban twins, Amphion and Zethos, rescue their mother, Antiope, who was held captive by Lykos, and who was placed in the care of the latter's wife, Dirke, who tormented her captive. The liberating twins punished Dirke by tying her hair to the horns of a bull and letting her be dragged to death. Similarly, the twin brothers, Calais and Zetes, the Boreadae, liberate their sister, Cleopatra, who had been a captive of Phineus. The latter had delivered Cleopatra to his wife, who tormented her captive. As in the other traditions, the liberating twins punished the tormentress. Phineus, however, remained unpunished.

Not only do the Greek legends agree in essence with the rescue episode of *Kudrun*, but also in some of the details. In each of the above stories the heroine is placed in care of the mother, or wife of the abductor. This woman is a sadistic female who takes pleasure in tormenting and mistreating the sister of the twins. She thus represents a precise parallel to Gerlind in the German epic. It should also be noted that, in each of the Greek legends, the twins leave the abductor himself unpunished. This element also corresponds to the German epic in which the heroes not only leave the abductor unpunished, but forgive him his deed.

Locating the theme in Indo-Iranian mythological tradition represents a somewhat greater problem; however, here too similar episodes can be de-

tected. It has already been pointed out that the Vedic Aśvins are promised in marriage to their sister, Sūryā (RV 7.69.4), and that they become her joint husbands (RV 4.43.6). However, in the marriage hymn Sūryāsūktam (RV 10.85.9), she is given not to the Aśvins, but rather to Soma, a lunar deity. The very same series of events occurs in the Baltic folk songs, a discussion of which follows. The actual motif of the abduction and liberation of Sūryā is not found in the hymns, but there are allusions to such an occurrence.[31] In a folktale from the Motinala, it is reported that Suraj, the 'Daughter of the Sun', is abducted by the Agaria who intends to keep her for his wife.[32] In this tale, Suraj is obviously identical with the Vedic Sūryā, thus indicating that Vedic mythology once knew the myth of the abduction of the Sun Maiden. Furthermore, Uṣas, the dawn, who is frequently confused with Sūryā in the Vedic hymns, is, in essence, liberated every morning from darkness by the Aśvins.[33] One hymn even states specifically that she is abducted (RV 6.60.2). There is, moreover, a whole series of hymns praising the Aśvins for rescuing the quail from the jaws of a wolf.[34] L. Myriantheus has convincingly argued that this episode represents the twins freeing Uṣas from darkness.[35] Although such a solar interpretation can never be proven, it does not seem likely that such a large number of hymns would praise the liberation of the quail if there were not a more profound significance to the episode.

The theme may have found its way into Indian heroic tradition where a situation parallel to the polyandrous marriage of the Aśvins and Sūryā is found in the Mahābhārata. Here the princess Draupadī is the joint wife of the five Pāṇḍava brothers, two of whom are Nakula and Sahadeva, sons of the divine Aśvins. Stig Wikander has published an essay in which he established beyond doubt that this pair represents a heroic manifestation of the Divine Twins.[36] During the course of the epic, Draupadī is rescued a number of times, and once she is led home by Nakula on a "chariot brilliant like the sun" (*rathenādityavarcasā*).[37]

The central episode of the other great epic, the Rāmāyaṇa, may represent another manifestation of a Dioscuric theme. The famous epic tells of the two brothers, Rāma and Lakṣmaṇa, who are handsome youths who carry swords and bows and glowing armor, and who look like glowing flames, making others bright with the reflection of their radiance. The pair of brothers are remarkably different in character. Rāma is a handsome warrior, while his brother is unusually kind, just, and an obedient servant. This precise contrast is also to be found between Nakula and Sahadeva in the Mahābhārata, and has been shown by Wikander also to be true for the Vedic Aśvins.[38]

After Rāma wins the hand of the lovely Sītā, he is banished from the country. His brother and his wife agree to join the hero in exile. Forced to wear the humiliating dress of bark, and carrying a hoe and basket, the trio leave the country.[39] In spite of their humble garb, the trio ascends the "flaming chariot of gold" and departs in a manner reminiscent of the Vedic Dioscuric triad who orbit the heavens in their celestial chariot. Later the

two brothers and the princess are seen upon a raft crossing a body of water; a scene which is likewise reminiscent of the Vedic Dioscuric tradition. Although Sītā is the wife of Rāma only, the constant presence of the husband's brother indicates that originally a polyandrous relationship—as existed between Sūryā and the Aśvins—lay at the base of the epic.

During their exile, Sītā becomes separated from the two brothers and is abducted by Rāvaṇa who takes the lovely princess to the isle of Laṅkā where he hopes to make her his wife. Sītā, however, refuses to have anything to do with her abductor and is consequently afforded cruel and humiliating punishment.

With the help of Sugrīva and Hanuman, a pair of monkey kings, a great bridge is constructed over the sea, across which Rāma and Lakṣmaṇa lead the liberating army. After many adventures the two brothers succeed in liberating the lovely Sītā who has suffered great torment in protecting her innocence. Toward the end of the epic, Sītā gives birth to a set of twin boys, who may represent a new incarnation of the Divine Twins.

Although the Dioscuric theme does not appear to have become part of a widespread oral tradition, there are a few isolated occurrences of the episode in popular folktales. In a Bengali folktale published by F. B. Bradley-Birt,[40] it is told that the youngest pair of seven brothers jointly liberate a lovely princess, who is subsequently abducted from them by the five older brothers. The twins eventually gain back the princess and punish the brothers. As in the Mahābhārata, the twins are youngest of a set of brothers, and occupy an inferior station. In the Bengali tale their position has acquired the tone of the mistreated 'stepchild' theme, typical of the folktale. The typically Dioscuric traits, however, are many: The twins appear suddenly to perform rescues at sea, they travel in their boats in the sky, they both court the hand of one princess, and, at the end, the brothers are 'shining' and 'god-like' and are envisioned on horseback.

Elements of this mythological theme can likewise be found in an isolated folktale recorded in Rumania (Aath 368C). The tale, which has been recorded in several Rumanian variants,[41] tells of a cruel mother-in-law who sends her daughter-in-law to the river in March to wash wool clean. Two wanderers, God and St. Peter, appear and help the maiden in her task. They also deceive the tormentress so that she follows flocks of sheep into the mountains and consequently freezes to death. The daughter-in-law is thus liberated from her torment.

It should be stressed that this tale is not part of a widespread oral tradition. Other than the Rumanian variants, the tale is unknown.[42] Of particular importance are the washing motif and duality of the divinities who help the woman in distress. The image of the maiden washing at the seashore occurs in Kudrun, and will also be shown to be associated with the Baltic Sun Maiden. The appearance of two divine figures is an indication that this tale was founded on a Dioscuric mythological theme. In order for the tale to be acceptable in a Christian society, however, the Divine Twins had to be supplanted by Christian figures. It was thus that God and St. Peter,

already well-known for their frequent epiphany and their granting of aid to the deserving, became a part of this tradition.

Elements of this mythological theme can likewise be detected in Latvian folk songs. The images described in these songs have clearly evolved from a solar mythology, and the recurring theme is the rising and setting sun with its accompanying phenomena.[43]

As in the Vedic tradition the Sun Maiden is a passenger in the boat of the Divine Twins:[44]

358-33732 I make a boat out of an apple tree,
Both ends are golden.
God's sons, the oarsmen,
Take the Sun Maiden sailing.

Just as in the Vedic tradition, the twin deities are reported to be the suitors of the Sun Maiden:

389-34008 The silver cocks are crowing
At the banks of the golden stream;
They are waking the Sons of God,
Suitors of the Sun Maiden.

In some instances, a single Son of God is reported to be the suitor:

368-33801(2) Whose grey steeds
Before the door of the Sun's house?
They are the horses of the Son of God,
The Sun Maiden's suitor.

The appearance of a single Son of God is unquestionably the result of a secondary development. Originally both deities were the suitors, and they were reduced to a single figure when the mythological polyandrous theme was no longer understood. This assumption is confirmed by the fact that two horses are mentioned in the above song.

As in the Vedic hymns, the Sun Maiden was promised to the Son(s) of God, but given instead to the Moon.

372-33843 While my mother was bringing me up
She promised me to God's Son;
But when I grew up, She gave me not [to him],
She gave me to the Moon.

That the Sun Maiden is speaking in this song becomes evident when one compares this variant with one published by Mannhardt.[45]

Die Sonne zog ihre Tochter gross,
Versprach sie dem Gottessöhnchen.
Als die gross gewachsen war,
Gab sie sie nicht, sondern gab sie dem Monde.

Another song reports that the Divine Twins become the joint husbands of the Sun Maiden(s).

362-33763 God's Sons and Sun Maidens[46]
Were celebrating a celestial wedding;
The Moon as he was moving,
Exchanged the rings.

In one verse it is revealed that the Sun Maiden has been abducted.

384-33950 Today the Sun is moving
More warmly than on other days;
Today someone has taken the Sun Maiden
From the Daugava to Germany.

Another song tells that it was the Moon who stole the Sun Maiden from the Morning Star.[47]

384-33950 The Sun shattered the Moon
With a sharp sword,
Because he stole the bride
From the Morning Star.

Often the Sun Maiden is envisioned at the seashore washing her face, golden pitchers, or white linen.

387-33984 The Sun Maiden beats the linen,
Upon an island in the sea;
A shirt of silk, a mallet of gold,
A washboard of silver.

There is a curious parallel to this washing motif in Bengali literature. In *The Manasa-mangal of Ketakā Dāsa* it is told that Behulā, who is the mortal incarnation of the Vedic Goddess Uṣas, sister of the Divine Twins, washes the clothing of the gods at the seashore to gain their favor.[48] The fact that the washing motif can be associated with the sister of the Divine Twins in both the Latvian and Indic traditions is an indication that it belongs to the original stratum of Indo-European mythology.

Various Latvian dainas report that the Sun Maiden is directed to rise early and perform some cleaning tasks, for on the following day the Heavenly Twins are expected to arrive.

391-34931 Arise early Sun Maiden,
Scrub the Linden table white;
Tomorrow morning the Sons of God are coming
To roll the golden apples.

It will be remembered that the heroine of *Kudrun* received a similar order on the day that her liberators were to arrive.

In some verses the Sun Maiden is seen at the seashore with her sister and her maidservant (403–55402). And in the following song the Sun is reported to have thrown silk into the sea:

382-33920 The Sun went to play
With the water in the sea;
The Sun was throwing silk,
The sea a piece of foam.

In other verses it is reported that the Moon weeps woefully because the Sun Maiden drowned while washing golden pitchers (374-33847; 405-55073). However, another verse reveals that the Sun Maiden did not drown, but that she was rescued by the Divine Twins:

386-33969 The Sun Maiden was wading in the sea;
Only her crown was visible.

Row your boat, O Sons of God,
Rescue the Sun's soul.

Although there are numerous additional songs involving the rescue theme, the above selection will suffice for the purpose of this investigation. It should be stressed that these verses constitute an arbitrary selection from a rather large corpus of mythological material. If a motif complex appears to develop, it is probably to be attributed to the choice and arrangement of the material. Although it would be convenient to hypothesize a Latvian myth from which these motifs have evolved, the investigator has no right to assume that such a myth existed on the basis of these songs alone. Nevertheless, the motifs are genuine, and their existence confirms the contention that the theme of the courtship and liberation of the Sun Maiden by the Divine Twins was once a part of Baltic mythology, and that it continued to live in Latvian popular tradition. Moreover, this theme is nearly identical to the other treatments of the abduction found in the mythologies of other Indo-European peoples. It is evident that this theme was a part of the corpus of Indo-European mythology at a time before the divergence of the various peoples from the original homeland. It may be well to summarize the elements of this theme as they are preserved in the various traditions (excluding Germanic):

a. The Divine Twins, sons of the Sky God (Vedic, Baltic, Greek),
b. who are associated with horses (Vedic, Baltic, Greek),
c. court the Sun Maiden (Vedic, Baltic)
d. who is their sister (Vedic, Greek, Baltic).
e. She is promised to them in marriage (Vedic, Baltic, Bengali),
f. but she is given to, or is abducted by, another (Vedic, Agaria, Rāmāyaṇa, Greek, Baltic),
g. who can be associated with the Moon (Vedic, Baltic).
h. The maiden is delivered to the mother of the abductor (Greek, Bengali).
i. She is forced to perform humiliating tasks or is otherwise tormented (Rāmāyaṇa, Greek, Baltic, Rumanian).
j. The maiden washes clothes at the sea (Baltic, Rumanian. The motif is also associated with the Vedic Uṣas but in another mythological context).
k. A pair of rescuers arrive (Rumanian, Rāmāyaṇa, Bengali, Baltic, Greek),
l. who are her betrothed and/or brothers (Rāmāyaṇa, Bengali, Greek, Baltic),
m. and who arrive by sea (Rāmāyaṇa, Bengali, Baltic. Moreover, the Vedic Aśvins and the Greek Dioskouroi are known particularly for their rescues at sea).
n. The captive maiden casts the clothes into the sea (Baltic).
o. The tormentress is punished by the rescuers (Greek, Bengali).
p. The abductor, however, remains unpunished (Greek).

It must be emphasized that this outline is not being posited as a reconstruction of an Indo-European myth, but has been made merely to afford a

quick glance at the theme as it appears in the various traditions. Furthermore, it must also be stressed that the Vedic and Baltic traditions offer no plot outline but merely isolated motifs.

If the above outline is compared with the rescue episode in *Kudrun*, the agreement is immediately striking. However, there is one significant difference. In the Indo-European tradition the liberators are the brothers and joint fiancés of the maiden in distress. In the German epic one of the brothers is the fiancé, while the other is her brother. Such a change is not only possible, but necessary. When the theme left the realm of mythology to become part of the heroic tradition, the themes of polyandry and incest would no longer have been understood. Thus the logical solution was reached. One of the twins retained the role of brother, while the other retained the role of suitor.

An analysis of some of the details of the epic reveals additional parallels with the Indo-European tradition. For example, stanza 1164 reports that, on the evening before the rescue, Ortwein and Herwig are looking to the west over the seas toward Ormanie where Kudrun was being held captive:

> ez was nu worden spâte, der sunne schîn gelac
> verborgen hinder wolken ze Gustrâte verre.
> des muose noch belîben Ortwîn und Herwig der herre.

('It had now become late, the glow of the sun lay/Hidden behind clouds in distant Gustrâte./Thus Ortwein and Lord Herwig had to remain.')

Gustrâte is evidently derived from *Guldstrâte* 'pathway of gold'. Thus the image is that of the last rays of the setting sun sinking in the water and leaving a path of gold toward the horizon. The image is reminiscent of the vision revealed in the Latvian songs in which the Sun Maiden sinks into the water, only her crown being visible as the twins approach by boat to rescue her. It is also reminiscent of the rescue episode in the Rāmāyaṇa in which a special bridge is constructed over the sea to Lankā, over which the brothers cross to liberate the captive Sītā. Furthermore, one is also reminded of the epithet *hiraṇyavartmanī* 'golden-pathed' borne by the Aśvins of the Rigveda.

Stanza 1220 of the German epic includes a passage which is somewhat cryptic. Herwig offers greetings of 'good morning' to Kudrun to whom '"guoten morgen" und "guoten abend" was . . . tiure.' Thus after a long separation the greetings of 'good morning' and 'good evening' are especially dear when the trio is reunited. That morning and evening are stressed may represent the survival of an older religious concept, for the Heavenly Twins were envisioned at dawn and at sunset in their celestial orbit leading the Sun Maiden home. Moreover, the Vedic triad of deities was invoked generally at dawn and in the evening.[49] The passage of the Middle High German epic may be one of the kind that preserves long-forgotten religious elements.[50] The tenacity of such a passage is demonstrated by the Kudrun ballad collected in the German speech island of Gottschee (Yugoslavia) in the nineteenth century. After the two rescuers say 'good morning' to the maiden who is washing at the seashore, she replies: *Lai guət moarn hon i a beank* 'Many good mornings I have few.'[51]

Similarly, stanza 1371 may have preserved elements of the original

Dioscuric theme, for in this scene Hartmut, the abductor, is watching the liberating army approach his castle when he notices the banner of Ortwein, *mit liehten sparren rot* 'with bright red wooden beams'. It is thus revealed that the banner of Ortwein, the liberating brother of the heroine, bears the symbol of wooden beams, the traditional aniconic idols of the Indo-European Divine Twins.

Moreover, in stanzas 1355–56 one learns that the arrival of the rescuing brothers is marked by the appearance of the morning star, a traditional symbol of the Indo-European twins, and by shining lights upon the beach, caused by the morning sun reflecting on their armor. Lights seen at sea are likewise associated with the Divine Twins of the Greek, Baltic, and Vedic traditions.

The agreement between the details of the German epic and the Indo-European tradition are too striking to be the result of mere coincidence. A genetic relationship unquestionably exists, and the next task is to determine the nature of this relationship. One could assume that the theme became part of a popular oral tradition, and, as such, spread across Asia and Europe. However, with the exception of the isolated Rumanian tale, which contains only a fragment of the theme, one will search in vain through both the Aarne-Thompson Tale-Type Index and the Thompson Motif-Index for the theme. Consequently there is no basis for assuming that the theme was ever a part of the oral narrative tradition of Eurasia. It is likewise obvious that the theme could never have spread at so early a date by a written literary tradition. Thus there remains only one tenable explanation. The theme was once part of the cult poetry of the Indo-European religion, and, as such, it was carried into the new homelands by migratory peoples, where it changed remarkably little in the new environments. Among the speakers of Germanic languages the mythological theme became a part of the heroic tradition, and it was just such a heroic euhemerization of a Dioscuric myth which served as a model for the author of the epic.

In view of these findings, it is now evident that the pantheon of the Germanic peoples knew the Indo-European Divine Twins, and that mythological themes associated with these twins were once a part of Germanic mythology.

Notes

[1] See F. G. Welcker, *Griechische Götterlehre*, 3 vols. (Göttingen, 1857–62), I, 606–15; A. Kuhn, *Die Herabkunft des Feuers und des Göttertranks. Ein Beitrag zur vergleichenden Mythologie der Indogermanen* (Berlin, 1859), see esp. 252–57.

[2] The songs of the Baltic region, the Latvian *dainas* and the Lithuanian *dainos*, offer excellent source material for the study of mythological themes. Although most of the songs were not collected until late in the nineteenth century, the traditions which these songs preserve is much older. Linguistic research indicates that the metrical pattern of many of the songs was already established at the time of the divergence of the Lithuanian and Latvian languages in the eighth century. See A. Švābe, Tautas dziesmu likteni, in *Latviešu tautas dziesmas*, eds. A. Švābe, K. Straubergs, E. Hauzenberga-Šturma (Copenhagen, 1952–56) I. v-xxxii.

[3] See e.g. G. Hopkins, *Indo-European* *Deiwṇos *and Related Words* (diss. Yale, 1932-Lang. Diss. 12).

[4] See *Latviešu tautas dziesmas* XI, Song No. 33732, p. 358. This is the most complete

collection of Latvian songs available, and, unless cited otherwise, the *dainas* used in this investigation are from volume XI of this collection. Hereafter they shall be cited solely by the page number and the *daina* number in that order. I am indebted to Mara Krisbergs, student at the University of California, Los Angeles, for the translations.

[5] For a detailed comparison of the various traits and functions of the Indo-European twin gods see, D. Ward, *The Divine Twins. An Indo-European Myth in Germanic Tradition.* Folklore Studies, 19 Berkeley and Los Angeles, 1968.

[6] *Die germanischen Runennamen. Versuch einer Gesamtdeutung* (Meisenheim am Glan, 1956) 322–400.

[7] *Hedenske Kultminder i norske stedsnavne. Skrifter utgit av Videnskapsselskapet i Kristiania, II hist. filos. klasse*, 2. bind. (Christiania, 1915), see esp. chs. VII-VIII and XX–XXII.

[8] *Undersökningar i germanisk mythologi* (Stockholm, 1886–89) II. 222–33.

[9] *La saga de Hadingus* (Saxo Grammaticus I, V–VIII), (Paris, 1953) 125–28.

[10] Studier till Sveriges hedna mytologi och fornhistoria, *Uppsala Universitets Årsskrift*, No. 8, Filosofi, språkvetenskap och historiska vetenskaper, 6 (Uppsala, 1924) 126–27.

[11] The various pairs of Indo-European Divine Twins were invoked to witness the swearing of oaths. See Ward 26–27.

[12] See *Den Norsk-Islandske Skjaldedigtning*, ed. F. Jónsson, I.106.

[13] Dumézil argues convincingly that Saxo's treatment of the saga of Hadingus consitutes a Viking retelling of the careers of the gods Njördr and Freyr. In the euhemerized version Hading represents Njördr, and Frotho (or sometimes Hunding) represents Freyr. See *La saga de Hadingus* 118–59.

[14] Neue Beiträge zum altgermanischen Dioskurenglauben, *Bonner Jahrbücher* 150.91–101 (1950).

[15] See J. de Vries, Altgermanische Religionsgeschichte, *Grundriss der germanischen Philologie*, XII, 1 and 2, 2nd ed. (Berlin, 1956–57) II, 247–49; K. F. Johansson, Germ. Alcis (germ. Dioskurer), *Arkiv för nordisk filologi* 35.1–22 (1919); H. Rosenfeld, Die vandalischen Alkes 'Elchreiter,' der ostergermanische Hirschkult und die Dioskuren *Germanisch-Romanische Monatsschrift* 28.245–58 (1940).

[16] See Ward 52–64.

[17] See R. Much, Raus and Raptus, *Zeitschrift für deutsches Altertum* 36.47 (1892). See also Ward 53.

[18] See *Norsk-Isländska Skaldedigtningen*, 2nd ed., E. A. Kock, 2 vols. (Lund, 1946–49) I, 323. See also N. Wagner, Dioskuren, Jungmannschaften und Doppelkönigtum, *Zeitschrift für deutsche Philologie* 79.1–17; 225–47 (1960).

[19] See F. R. Schröder, Nerthus und die Nuithones, *Die Sprache* 6.141 (1960).

[20] See F. Liebermann, Hengist und Hors, *Archiv für das Studium der neueren Sprachen und Literaturen* 151.79 (1927). See also O. Huth, Vesta. *Untersuchung zum indogermanischen Feuerkult, Beihefte zum Archiv für Religionswissenschaft* 2 (Leipzig und Berlin, 1943) 85. For illustrations of the heads, see Huth, *Sagen, Sinnbilder, Sitten des Volkes* (Berlin, 1942) 30–32.

[21] See N. S. Aurner, Hengest. A Study in Early English Hero Legend, *University of Iowa Studies, Humanistic Studies* 11.1 (Iowa City, 1921). See also K. Schreiner, Die Sage von Hengest und Horsa, *Germanische Studien* 22 (Berlin, 1921). Each of these works, both of which were published the same year, makes an excellent compilation of the numerous sources on the exploits of the heroic brothers.

[22] For examples see Ward 14–15, 40–44.

[23] Of the 41 sources cited by N. Aurner, seventeen include the episode of drawing lots.

[24] See Ward 27.

[25] A. Furtwängler, Dioskuren, in *Ausführliches Lexikon der griechischen und römischen Mythologie*, 10 vols., ed. W. H. Roscher (Leipzig, 1884–1937) I, 1.1159.

[26] See C. Brady, *The Legends of Ermanaric* (Berkeley and Los Angeles, 1943).

[27] A. H. Krappe, *The Legend of Rodrick* (Heidelberg, 1923) 30–59; M. Roediger,

Die Sage von Ermenrich und Schwanhild, *Zeitschrift des Vereins für Volkskunde* 1.241–50 (1891); H. Naumann, *Neue Beiträge* 97.

[28] The epic has been preserved in only one complete manuscript, a copy of the early sixteenth century (Ambraser) Heldenbuch. The poem itself, however, was composed in the thirteenth century.

[29] B. Symons, Heldensage, in *Grundriss der germanischen Philologie*, ed. H. Paul (Strassburg, 1893) II, 1.53–55; A. Heusler, Kudrun, in *Reallexikon der germanischen Altertumskunde*, ed. J. Hoops (Strassburg, 1911–19) III.113; H. Schneider, Ursprung und Alter der deutschen Volksballade, in *Vom Werden des deutschen geistes, Festschrift für G. Ehrismann*, ed. P. Merker und W. Stammler (Berlin und Leipzig, 1925) 118–19; Fr. Panzer, *Hilde-Gudrun* (Halle, 1901) 399–405.

[30] Supervivencia del poema de Kudrun, *Revista de filología española* 20.1–59 (1933), reprinted in German translation, Das Fortleben des Kudrungedichtes, *Jahrbuch für Volksliedforschung* 5.85–122 (1936).

[31] See L. Myriantheus, *Die Açvins oder die arischen Dioskuren* (München, 1876) 33.

[32] See V. Elwin, *The Agaria* (London, 1942) 100. n. 1.

[33] Furtwängler, Dioskuren, col. 1161–62, Krappe, *Mythologie universelle* (Paris, 1930) 81–82, and Myriantheus, *Die Açvins* 83, all speak of the liberation of Uṣas by her brothers, the Aśvins. However, the hymns do not specifically report such an incident.

[34] See *RV* 1.116.14; 1.117.16; 1.112.8; 10.39.13.

[35] *Die Açvins* 75–76: "Die Finsternis ist der Wolf, welche [sic] alles raubt und verschlingt, sie raubt abends die Aurora und verschlingt sie in ihren Rachen. In der Frühe jedoch befreit das Zwielicht [The Aśvins], welches den Tag herbeibringt, jene wieder aus dem finsteren Rachen der Höhle."

[36] Pāṇḍava-sagan och Mahābhāratas mytiska förutsättningar, *Religion och Bibel* 6.27–39 (1947). See also by the same author, Nakula et Sahadeva, *Orientalia Suecana* 6.66–96 (1957).

[37] Mhb. X, 11.577. Compare RV 8.8.2 in which it is reported that the Aśvins lead Sūryā home in a chariot which resembles the sun (*râthena sûryatvacā*).

[38] Nakula et Sahadeva 66–89.

[39] The humiliating treatment as well as the agricultural implements are reminiscent of the situation in the Rigveda where, as divinities of the common agriculturists and herdsmen, the twins are refused participation in the Soma sacrifice.

[40] *Bengal Fairy Tales* (London and New York, 1920) 139–49.

[41] See Schullerus, *Verzeichnis der rumänischen Märchen*, FFC 78 Type 368C, Baba Dochia.

[42] The so-called Kudrun ballads treat a similar theme; however, the ballads are more closely related to the Kudrun epic than to the Rumanian folktale.

[43] Although it is not too difficult to determine the precise phenomena which evidently gave rise to the mythological themes, there is little need to do so for the purposes of this study. See W. Mannhardt, Die lettischen Sonnenmythen, *Zeitschrift für Ethnologie* 7.73–104; 209–44; 281–329 (1875).

[44] See note 4 above.

[45] Die lettischen Sonnenmythen, 82, Song No. 72. Some of the dainas report that the Sun promised her daughter to the 'Sons of the Morning Star' (383–33946).

[46] The fact that more than one Sun Maiden is mentioned is doubtless the result of a secondary development, that is, another means of solving the problem posed by the polyandrous theme.

[47] The Divine Twins have traditionally been linked with the Morning and Evening Stars in various Indo-European mythologies. See Ward 59–69.

[48] Edward C. Dimock, Jr. (trans.), *The Thief of Love* (Chicago and London, 1963) 271–72.

[49] *RV* 8.22.14; 10.39.1; 5.77.2; 10.40.2; 10.40.2.

[50] As an example of the longevity of such a concept, compare the English expression 'by Jiminy!' which is the survival of a pagan oath-swearing formula, 'by Gemini', and which is still very much current.

[51] See A. Hauffen, *Die deutsche Sprachinsel Gottschee* (Graz, 1895) 249.

The Quantitative Meter of Indo-European

Jerzy Kuryłowicz

It seems that in spite of the numerous attempts at reconstruction of IE metrical patterns, the proper object of such a reconstruction has not been clearly envisaged. Proof of related meters has been looked for in the number of syllables and in the configurations of rises and falls ('temps forts' and 'temps faibles'). This is a serious methodological derailment caused by a direct and superficial carrying over of points of view of comparative grammar into prosody. Another was the confusion between 'metrical' and 'poetical' when trying to prove metrical relationship by pointing out similar word-collocations occurring in the poetry of related languages. Whereas poetical language concerns SEMANTICS in the first instance, metrics is closely bound up, though as we shall see indirectly, with the PHONEMIC system of the language.

In language we have a plane of content and a plane of expression, the latter representing the implement, and the former the goal. We speak of the function of communication, representation, symbolization of language. Here we know exactly the goal and the instrument by which it is achieved.

Similar or identical semantic and syntactical categories occur in languages which are not necessarily related. Identity of semantic or of syntactical function is therefore an insufficient proof of a genetical relationship between two languages. In order to prove HISTORICAL affinity we must show that the material implementation is similar or rather, in the last instance, identical. To put it crudely we must establish so-called phonetic laws.

What is being aimed at in the meter? Answer: metrical patterns, i.e. certain configurations of rises and falls, their number, etc. Just as the linguistic categories, they are represented by types occurring in various related and unrelated languages, being more or less common. Thus, e.g. a verse-line of 8–12 syllables is more common than a shorter or a longer one, this being only a consequence of man's limited capability of rhythmical perception and integration. The arrangement of rises and falls, chiefly into iambs, troches, dactyls etc., into bars and feet, may be implemented by stress/lack of stress or by length and shortness of syllables. Patterns often fluctuate within the same language. Therefore the conclusion that the appearance in two IE languages of, e.g. decasyllables or hendecasyllables, with a partially iambic rhythm, catalectic or acatalectic, enlarged or shortened, etc., points to a common origin of such a metrical pattern, is no more justified than the as-

sertion of the common origin of the IE and the Semitic plural, genitive, or subjunctive.

The latent argument in such an assertion, i.e. in the traditional attitude, is of course that the same or similar metrical pattern appears in two or more RELATED LANGUAGES, say Indic and Greek. This is just the fallacious argument mentioned above, mechanically taken over from the comparative grammar. Phonetic equations may prove the historical identity of linguistic categories, take e.g. the ending *-m̥* of the accusative singular which has identical morphological functions in all IE languages. Phonetic equations cannot, however, prove the genetic identity of meters because the material EXPRESSING the metrical pattern is of a particular nature. Just as the meter is a superstructure on colloquial language, even so there are additional requirements imposed by the meter on the phonetic material of the language.

To express metrical patterns the phonetic material of the language must undergo a special treatment and even a certain transformation; cf. contractions, elisions, loss of word-accent, etc., a whole series of changes partially or totally unknown to colloquial language. In order to prove the historical relationship of meters we must analyze these transformations, which are the DIRECT material serving to express the metrical patterns. The phonemic structure of the colloquial languages has only an INDIRECT bearing upon the meter.

The following remarks aim at showing that the two most striking features of the Indic and of the Greek meter, viz. quantity as rhythmical factor and a special metrical sandhi, are not independent of each other, the former being only a consequence of the latter. This fact raises the question whether the two kinds of verse-internal sandhi, as represented by Indic and Greek versification can be reduced to a single pattern and be used as an argument in favor of a common heritage. It raises, moreover, the question whether the origin of the quantitative meter is to be attributed to IE or due to individual, independent developments.

Vedic versification shows an interesting trait proving the autonomy of metrical structures and their independence from syntax. The personal verb of the main clause as well as the vocative are accented at the beginning of the pada, just as in the colloquial language they are always accented in the beginning of the sentence. But this peculiarity seems at the same time to indicate a transposition of the facts of the spoken language into metrics. Whatever its origin (it would be otiose to try to trace it back to verse = sentence), functionally the cut between two subsequent verses is appreciated, in comparison with the caesura and other secondary breaks within the verse-line, as extreme, just as the cut between two sentences (full stop versus comma, etc.). The accentuation of the verb and of the vocative in the beginning of the verse is the material exponent of the preceding cut. This accentuation is not prevented by a possible enjambement, i.e. by a syntactical nexus joining the two verse-lines. Nor does sandhi combination take place in spite of syntactical closeness.

One could go one step farther and consider the metrical foot as a trans-

position of the word, the articulation of the verse (into feet) being *mutatis mutandis* similar to that of the sentence (into words). Real homomorphism, however, begins at a lower level, that of the syllable, a constituent part of the word as well as of the foot. Both of them are syllabic complexes characterized by the 'culminative' function of one of the syllables, carrier of the accent or ictus, respectively.

Podic structure is not, however, an inherent feature of the quantitative meter. On the one hand we find nonpodic quantitative meter, e.g. in Vedic, on the other hand podic meter can occur in accentual metrics (cf. the bars (*Takte*) of the Germanic *Langzeile*).

A more promising approach to homomorphism due to the transposition of linguistic devices to metrical structures is the comparison of hemistichs with syntactical groups. In quantitative metrics sandhi combinations within the hemistich are obligatory just as within closeknit syntactical groups of the spoken language. The caesura, on the other hand, is comparable to the juncture dividing such syntactical groups, i.e. prevents sandhi combinations, at least originally. In the later periods, in spite of the weakening of the caesura, sandhi combinations at the caesura are much rarer than in all the other positions of the verse-line.

According to Arnold (*Vedic Meter* 71) the hiatus is relatively more frequent at the caesura than elsewhere. On the other hand, sandhi combinations occurring at the caesura are almost unknown at the end of the verse (as a matter of fact there is only one probable instance of sandhi combination at the end of the verse, whereas six others are uncertain; p. 72).

Some figures are indicated to illustrate the relative frequency of the absence of sandhi combinations at the caesura. Let us take the most striking one, hiatus versus contraction (Arnold p. 73–5). In the hendeca- and dodecasyllable of the Rigveda the caesura is only one of the ten or eleven places within the verse where hiatus can occur, so theoretically there is only a 1:10 or 1:11 chance of finding the hiatus at the caesura. But, as a matter of fact, we find:

antevocalic -*ă* occurs 113 times at the caesura, 146 times in other positions

antevocalic -*ā* occurs 96 times at the caesura, 98 times in other positions

antevocalic -*ĭ*, -*ŭ* (*praśliṣṭa*) occurs 25 times at the caesura, 21 times in other positions

Judging by the frequency of hiatuses occurring in other positions we should expect, *caeteris paribus*, something like 15, 10, and 2 instances, respectively, at the caesura. It seems therefore that the caesura prevents the application of sandhi rules, though to a lesser extent than the end of the verse. Examples of a similar action of the caesura are also found in the dactylic meter of the Classical languages. Cf. Westphal, *Allg. Metrik* 1892, 373f.:

trochaic: A 569 *kaì rh' akéousa kathêsto/epignámpsasa phílon kẽr*
B 315 *mẽtēr d' amphepotâto/oduroménē phíla tékna*

bucolic:	B 218	*kurtṓ, epì stêthos sunokhōkóte;/autàr húperthe*
	E 221	*all' ág' emō̃n okhéōn epibḗseo,/óphra ídēai*
penthemimeres:	A 34	*all' ouk Atreídēi/Agamémnoni hḗndane thumṑi*
	B 451	*otrúnous' iénai./en dè sthénos ō̃rse hekástōi*
trithemimeres:	A 441	*patrì phílōi/en khersì títhei kaí min prosḗeipen*
	I 341	*Atreídai?/epeì hóstis anḕr agathòs kaì ekhéphrōn*
hephthemimeres:	B 809	*pâsai d' ōígnunto púlai,/ek d' éssuto laós*
	ζ 77	*pantoíēn, en d' ópsa títhei,/en d' oînon ékheuen*

The first four instances illustrate the preservation of short vowel before vocalic initial.

The rest show the preservation of length in diphthongs preceding a vocalic initial.

Similarly in Latin. Cf. (Koster, *Traité de métrique* 1953, 314): trochaic: Vergil *Buc.* 2,53 *Addam cerea pruna;/honos erit huic quoque pomo*; bucolic: *Aeneid* I, 405 *Et vera incessu patuit dea;/ille ubi matrem*; penthemimeres: *Aeneid* I, 651 *Pergama cum peterĕt/inconcessosque hymenaeos*; trithemimeres: *Aeneid* III, 606 *Si pereo,/hominum manibus periisse iuvabit*; hephthemimeres: *Aeneid* III, 464 *Dona dehinc auro gravĭă/sectoque elephanto* (notice the function of *a* of *gravia* as *anceps* as at the end of the verse.)

The weakening of the caesura, observable in the historical period, entails the spread of sandhi combinations across the whole verse-line. It is the lack of sandhi at the caesura which in the descriptions of the classical meter is considered as exceptional. But it may be safely assumed that originally the caesura must have been much more pronounced and characterized by the absence of sandhi combinations.

This development is of course in agreement with the gradual emancipation of the metrical structural forms from those of the colloquial language. Just as the hypothetical correspondence verse = sentence is given up owing to the development of enjambement, even so the correspondence hemistich = syntactical group (delimited by junctures) disappears owing to verse-internal enjambement, i.e. to the dissociation between caesura and syntactical juncture. The end of the verse-line as well as the caesura become, more and more, syntactically meaningless.

In the colloquial language sandhi occurs as a rule only between words forming closeknit syntactical groups. Cf. Classical Greek prose, where elision of final antevocalic vowel is proper chiefly to prepositions and conjunctions. A parallel state of affairs is to be assumed for O. Indic and Middle Indic (Wackernagel, *Altind. Gramm.* I. 308), though here the sandhi of the written prose has been doubtlessly influenced by the model of the holy texts. Some scanty traces of sandhi in the colloquial language of Latin are borne out by old 'phrase compounds' like *anim-advertō*, *vēn-eō* (elision of nasalized vowel), *n(e)-ullus*, *n(e)-umquam*, *non* < *n(e)-oinom*: cf. also *bonus 'st*, *bonum 'st*.

As against this moderate use of sandhi combinations in the colloquial language the latter are prevented in the meter only by the end of the verse and by the caesura.

Following are the systematic deformations of word-forms and the changes of syllabication brought about by the obligatory metrical sandhi in Greek (*E* denotes any vowel, *T* any consonantal element):

-E before *E-* is elided, e.g. *rha akéousa* > *rh'akéousa*

-Ē (or diphthong) before *E-* is shortened, e.g. *ámbrotoi (h)oùs* (-˘˘-)

-E + T_1T_2- becomes *-ET_1 + T_2-*, e.g. *epì stēthos* > *e-pis-tē-thos*

-T + E- becomes *TE-*, e.g. *oînon ékheuen* > *oi-no-ne-kheu-en*

-T_1T_2 + E- becomes *-T_1 + T_2E-*, e.g. *ex ára* > *ek-sa-ra*

The conditions in Sanskrit and in Latin are similar, though not identical. The differences, however, are not relevant to the issue. The chief one, between Sanskrit and the Classical languages, lies in the treatment of vowel before vowel. The metrical elision in Greek is paralleled by a similar phenomenon occurring in compounds, where it has been shown by Wackernagel (in his famous treatise *Das Dehnungsgesetz der griechischen Composita* 1889) to be nothing else than a transformation of the Indic contraction. The Greek reinterpretation of the contraction as ELISION OF THE FIRST PLUS LENGTHENING OF THE SECOND VOWEL was due to the rise of internal hiatuses after the disappearance of internal *s*, *y*. The final stage, i.e. elision WITHOUT lengthening, is a corollary of the first contractions (cf. *L'apophonie en indo-européen* 1956, 264–6). We may fairly assume that the difference between the metrical sandhi of Skt. (Vedic) and that of Greek, viz. contraction versus elision, is the result of a similar development.

The chief thing, however, is the generalization of sandhi combinations, of limited occurrence in the colloquial language. Its function is of course to integrate the hemistich or the verse-line as a metrical unit opposed to other units of the same kind and to delimit it from them by a metrical juncture, i.e. the absence of sandhi. But admitting this function, we still lack the evidence of an organic transition from the rather limited phenomenon of the colloquial language and the rigorous metrical rules disregarding syntactical junctures and entailing phonemic deformations.

Cohesion within a syntactical group entails certain changes in the accentuation of its members. As a rule hierarchy of accents is the consequence of syntactical cohesion (e.g. *dark*[3] *clouds*[4]). The degree of accentual subordination may be different, the extreme case being loss of accent on the part of one of the members, which becomes enclitic or proclitic, e.g. *with things* (*with* proclitic) as against *things I can dispense with* or *give him* (*the book*) with *him* enclitic versus *give it to him*. There must have been, both in Indic and in Greek, a scale of accentual hierarchies depending on the degree of cohesion of the syntactical group and, of course, on the character of its members. But all these varieties were only predictable phonemic (or rather prosodic) combinatory variants. A palpable phonemic trace, however, appeared in the extreme case of the loss of accent of a member, such a loss entailing word-internal sandhi, i.e. contraction or elision, respectively, and shift of syllabication. Word-internal sandhi was, therefore, the only phonemic means of stressing the internal cohesion of a metrical unit, whether hemistich or verse.

The immediate consequence of this generalization of word-internal sandhi is unexpected though necessary. Since internal sandhi presupposes loss of accent due to the integration of the syntactical group, its generalization within the hemistich or within the verse involves the elimination of all individual word-accents. Here we touch the rock bottom of the phenomenon called quantitative meter. The metrical sandhi proper to Indic and to the Classical languages, suppressing all syntactical junctures, eliminates at the same time accent, the natural vehicle of the rhythm in the colloquial language. The distinction between accented and unaccented syllables ceases to exist and a subsidiary distinction between heavy (long) and light (short) syllables may take over, i.e. become the carrier of the metrical rhythm. This can of course happen only in languages where the vocalic quantity being phonemic permits the establishment of a neat distinction between $T\breve{E}$ and $T\bar{E}$, $T\breve{E}T$, based on the equivalence $T\breve{E}T = T\bar{E}$.

We may put it like this: in colloquial speech there is first a delimitation of words (due to word-accents), then within the *unaccented syllables* of the word a difference between long and short syllables (in order to contrast a long accented and a short accented syllable we should be obliged to have recourse to *two words*). It is only the obliteration of word accents, due to the suppression of word-junctures, that puts all syllables of the verse (or hemistich) *on a par* (all being henceforth 'unaccented') and permits one to oppose all long syllables to all short ones.

We have to do with a transition from the 'natural' rhythm of accentuation, proper to the colloquial language and to numerous systems of versification, to an 'artificial' metrical rhythm based on another prosodic feature of the word, the quantity of its syllables.

The reconstruction of this transition can be only hypothetical. The introduction of the syntactically unmotivated sandhi was aimed at the strengthening of the metrical cohesion of the verse-line. Assuming that originally (as e.g. in Old Germanic or in Avestan) the meter depended on word-accent, the artificial sandhi must have endangered its position as rhythmical factor—or rather a gradual rise of the quantitative ingredient paralleling the simultaneous decline of the accent (due to metrical sandhi). It is quite possible that in certain crucial positions, cadence or caesura, the temps fort was originally represented by a long accented syllable, not simply by any accented syllable. Cf. the Old Germanic meter, where the contrast between temps faible and temps fort is rendered not simply by UNACCENTED : ACCENTED, but by UNACCENTED : LONG ACCENTED (or its equivalent). The above elimination of word-accents, brought about by the metrical sandhi, would have resulted in verse-lines provided with syllabic length at crucial places, forming the starting point of a new, quantitative meter.

The distribution of quantity, as attested in the oldest quantitative meters, seems to bear out this hypothesis. In the Vedic octosyllable only the sixth and the seventh syllable, belonging to the cadence, show a fair degree of regularity, the former being long in about 77.5 percent, the latter short in over 80 percent of verses. On the other hand, the structure of the FIRST

HALF of the verse-line (syllables 1–4) is unpredictable as regards the repartition of quantity. Cf. (Arnold *op. cit.* 153):

⏑̆---	35.8 percent of verses
⏑̆-⏑-	25.6 percent of verses
⏑̆⏑--	14.4 percent of verses
⏑̆--⏑	10.8 percent of verses
⏑̆-⏑⏑	5.0 percent of verses
⏑̆⏑-⏑	5.5 percent of verses
irregular	2.9 percent of verses
	100.0 percent

In the hendeca- and dodecasyllables the cadence is nearly predictable. In 98.2 percent of cases the cadence comprises ⏑-(⏑)⏑̆, i.e. ⏑-⏑̆ in the hendecasyllable, ⏑-⏑⏑̆ in the dodecasyllable. Here again, however, the dispersion of the quantity in the first part of the verse (with caesura after the fifth syllable) renders impossible the establishment of any rule regarding the quantity of individual syllables:

⏑̆-⏑--	29.3 percent of verses
⏑̆-⏑-⏑	19.8 percent of verses
⏑̆----	18.0 percent of verses
⏑̆---⏑	15.4 percent of verses
other forms	17.5 percent of verses
	100.0 percent

In Greek we find a similar phenomenon, the Aeolic basis of Sappho and Alcaeus viz. ⏑⏑̆ (i.e. indiscriminately --, -⏑, ⏑- and ⏑⏑) representing the first part of different meters. For other responses of this kind cf. P. Maas and W. Lloyd Jones (*Greek Meter*, 1962, 25–8).

Be this as it may, our chief contention here is the stressing of the role of the artificial (i.e. nonsyntactical) sandhi in ousting the accentuation of the word as the vehicle of the metrical rhythm. From the genetical standpoint the metrical sandhi is to be regarded as the primary phenomenon, quantitative meter as its consequence. From the descriptive point of view this kind of sandhi may of course be considered as a nonessential peculiarity of the quantitative meter.

At this point two important questions must find their answer:

(1) Is the quantitative meter of other languages (Persian, Classical Arabic) to be explained by the same principle?

(2) Are the similarities between Indic and Greek to be considered as an IE heritage?

(Ad 1) As regards the Persian meter, there are two metrical sandhi phenomena which represent a generalization of word-internal sandhi. In compounds an extra syllable (short or reduced vowel) is inserted between a heavy syllable (= in double consonant or in long vowel + consonant) and the second member beginning with a consonant. Now the same rule is applied to ANY two contiguous words in the verse-line, without regard to the

syntactical nexus. Furthermore, a final syllable consisting of short vowel + consonant may count as a short syllable before the initial vowel of the following word.

Concerning Arabic, the pertinent phenomena have been analyzed in the Marcel Cohen volume, still to appear.

(Ad 2) In trying to establish the historical affinity of Indic and Greek meters we must analyze the metrical sandhi phenomena, i.e. the phonemic structure of the verse-line, in the first instance. It is clear that only agreement and identity established on the basis of such MATERIAL CLUES, not numbers of syllables, iambs, dactyls, etc., will permit us to speak of INDO-EUROPEAN versification, i.e. of the versification of people speaking a more or less defined language. Let us look at the method of establishing the relationship of the IE languages. The fact of their sharing the categories of plural, genitive, perfect, etc., is not sufficient to prove their affinity. The decisive factor is the (indirect) agreement of the phonemic expressions of these categories. We may compare abstract rhythmical schemes consisting in certain arrangements of temps forts and temps faibles and in certain numbers of syllables to such abstract grammatical categories. The internal structure of the verse-line, based on the metrical exploitation of the phonemic and prosodic features of the language, would then correspond to the material realization of the categories, to phonemic shapes, and would be the only admissible criterion for proving historical relationship.[1]

We have seen that the Greek elision (lengthening) may be safely traced to old contraction. This does not necessarily mean that the Greek and the Indic contractions, as well as the shifts -T + E- > TE- etc., are both inherited, even putting aside the hypothetical existence of initial laryngeals. Still there is at least a noticeable parallelism of development to be considered.

The counterpart of the Indic development is represented by the Gathas of the Avesta, whose meter is probably based on word-accent. The most striking feature of the quantitative meter, the metrical sandhi, is here totally absent. Contraction does not simply occur, not only between adjoining words, but also between members of compounds and even between stem and suffix (cf. *Le gén. en (n)ām en indo-iranien* Biuletyn PTJ 1962, 93ff.). A neat delimitation of word-forms seems to be the rule. But here again it would be difficult to decide the question of the relative chronology of this metrical pattern. In the last instance meter is directly or indirectly rooted in the colloquial language, and falling back on the natural rhythm of word-accent is an important means of rejuvenating the meter. Therefore it is hardly possible to answer the question whether compared with the Vedic meter that of the Gathas is an archaism or represents a more recent development.

One may justly regard the accentual meter as being more natural, the quantitative as being more artificial (without the depreciative shade of the term). The former is nearer to language in its fundamental, i.e. colloquial form. The latter is the result of a further, internal, evolution of the meter,

conditioned by the factors analyzed above. The artificiality of the classical meters seems evident to the modern reader familiar with the Romance, Germanic, Slavic versification, but this tacit opinion may be at the same time regarded as objective. The artificiality is still enhanced in podic meters, which introduces a secondary articulation of the verse (feet) cutting across word boundaries.

It is the filiation natural rhythm (of the spoken language): accentual meter : quantitative meter that is important and permits us to account for the rise of the Indic and of the classical meters. As for the question of a common IE origin of the Indic and the Greek verse, it must remain open. It seems preferable to state the essence of the problem rather than to submit a gratuitous hypothesis based on some trite and superficial similarities.

Notes

[1] Notice once more that the usage of related linguistic forms or of etymologically related vocabularies is not a sufficient argument proving the common origin of meters. Real proof consists only in showing a common materies metrica, transformed or even deformed, for metrical purposes. To rely upon language as such would be just another methodical derailment. The gist of the problem lies in the contrast colloquial: metrical.

Recently it has been observed by W. S. Allen that in Greek it was only the last heavy syllable of the word which could bear the ictus before the verse-end (or the caesura). Cf. *Transactions of the Philological Society* 1966, 107–148: Prosody and Prosodies in Greek. This observation is favored by statistics, by a majority of cases going up to 90–95 percent (p. 122).

The above fact is interpreted by the author as a proof of a correlation existing between ictus and linguistic stress.

There is, however, no metrical rule, comparable e.g. to ⏑ = -, which as such would have been directly perceived both by the ancient poets and the modern scholars. It seems rather that statistics points to the survival of such a correlation. It is probable that in certain metrical key-positions (chiefly in the cadence and the caesura) stress, the original carrier of the ictus, has been reinforced by quantity.

But two points must be taken into account:

1) The difference between the *natura longa* and *positione longa*: the metrical role of the heavy syllable is the result of an extension from the former to the latter.

2) The phonological definition of the Greek accent which falls either on the "final complex" (– = $\overset{\cup}{-}$ ∪) or on the preceding mora:

a) xx . . . x$\acute{-}$ = xx . . . x$\acute{\cup}$

b) xx . . . x≃ = xx . . . ≃∪ = xx . . . $\acute{\cup}$∪

c) xx . . . $\acute{x}$x∪ = xx . . . $\acute{x}$- (recessive accentuation).

Recessive accentuation representing the neutral member of this system, only words belonging to a) and b) may be considered as being accented, and only xx . . . x$\acute{-}$, xx . . . x≃, xx . . . ≃∪ as being word-types with accented long syllable i.e. just the types which according to the above statistics bear the metrically most important ictus (before cadence or caesura) on their last long (>heavy) syllable.

As regards Vedic a trace of the old meter based originally on word-accent is perhaps suggested by the following fact:

The original absence of external sandhi permitted one to use final syllables of the type -*ắt* as arsis before word-initial vowel (*a*-). After the generalization of metrical sandhi and the simultaneous elimination of word-accent the shift of -*ắt* + *a*- to -*ắ* + *ta*- entailed the occasional survival of accented short syllables as the bearers of

ictus in the cadence ∪∪x instead of -∪x (in the octosyllable) or of (-)∪-x (in the hendecasyllable). Not less than 38 irregular cadences of the type *-ăt + a-* (*-ă + ta*) have been counted besides 163 instances where the first or second short vowel of ∪∪x is in word-internal position. The most frequent cases are represented by *avi/ava-* (32 x), *śavi/śava-* (20 x), *ajara-* (13 x), *mahi/maha-* (7 x), *ratha-* (7 x), *dadhi/dadha-* (7 x). (Cf. *Prace Filologiczne* 11 (1927) 239 ff., where an obsolete explanation of these facts is offered.)

Background 'Noise' or 'Evidence' in Comparative Linguistics: The Case of the Austronesian-Indo-European Hypothesis

Isidore Dyen

I will here use the term 'evidence' as that material which taken together is explained by (or, more plausibly, is dependent on) one hypothesis rather than another competitive hypothesis. Evidence thus viewed will be presented which might be regarded, when taken together, as explicable only on the basis of a common origin of the Austronesian and Indo-European families of languages.[1]

The writer is not persuaded that the evidence is sufficient to demand the hypothesis of a Proto-Austronesian-Indo-European (PAIE), but it is of sufficient size and quality to raise the question as to the possible magnitude and quality of a chance-based collection of evidence for a hypothesis of relationship. For if there was no Proto-Austronesian-Indo-European, the collection of evidence for it which I will present will bear on the amount and kind of 'background noise' that can be expected in hypotheses of common origins which involve great time-depth.

Although at first the discussion may appear to be part of a scientifically uninteresting game, I believe that at the very least it makes a contribution toward the refinement of the criteria of relationship in nonobvious instances. This cannot help but bear on the question of the next-of-kin of the Indo-Europeans and also, of course, of the Austronesians.

The hypothesis to be dealt with is not favored by considerations of the distribution of the two families. Indeed, the first contact between speakers of languages belonging to the different families is probably to be associated with the spread of Indic power and colonization to the east about the beginning of the first millennium A.D. The probable homelands of the respective families appear to be very distant; that of the Indo-Europeans is probably in Europe whereas that of the Austronesians is no farther west than the longitude of the Malay Peninsula in any reasonable hypothesis and has been placed considerably farther east in at least one hypothesis. The hypothesis suggested by the linguistic evidence is not thus facilitated by a simple homeland hypothesis. Rather a homeland hypothesis to account for the linguistic evidence, even if that should be regarded as persuasive, would face difficulties that could reasonably be regarded as counterevidence.

The linguistic evidence almost entirely takes the form of two kinds of

comparisons. One type consists of comparisons of Proto-Austronesian reconstructions with Proto-Indo-European reconstructions. The other type consists of comparing a proto-language reconstruction with a daughter language form in the other family. We cannot deny that at least occasionally forms appear in too few daughter languages to allow for persuasive reconstruction in their proto-language. The evidence has in every case been collected independently of any other scholar who may have proposed the same hypothesis (such as Bopp).

The original stimulus to this hypothesis at all was Greenberg's statement that in a table of the numerals of the Indo-European languages 'by the time the second word has been examined the correct hypothesis (of relationship—I.D.) emerges.'[2] At precisely this point I would say that Malay, by virtue of *dua* 'two', would be indistinguishable from an Indo-European language and the correct hypothesis would not have emerged. A second stimulus from an entirely different direction was given by Benedict,[3] who claims that the Thai family is related to Austronesian (under the name 'Indonesian') and presents an extensive argument which consists essentially of a collection of plausible cognate pairs. Now most roots in Austronesian have a generalized shape (C)V(C)(C)V(C) whereas those of the Thai language have a generalized shape C(C)VT(C) where T is 'tone'. Benedict forms plausible cognate pairs between the dissyllabic Austronesian roots and the monosyllabic Thai words by permitting either syllable of the Austronesian root to be selected and by ignoring the difference in tone.

Our practice is perhaps not much better. We shall not insist on systematic matching between vowel qualities and we shall also allow discrepancies in the matter of initial vowels. We shall also not insist on the systematic matching of more than two consonants. Whether we achieve an impressive array, I leave to the audience to decide.

On the other hand one can regard the canonical shape of a PIE root as CC(C), in which the nonsyllabics (C) were at one time separated by syllabics whose quality was wholly conditioned. It is not at all difficult to imagine that the distinctions in the vowels reflected by PA were lost in PIE and furthermore that the unmatched consonant in a PA root relative to a PIE root was also lost in the latter or reflected in laryngeals. To this extent anyway, Proto-Austronesian is as likely a relative of PIE as Proto-Hamito-Semitic for though the latter, like PIE offers a canonical shape of CC(C) for root morphemes, its characteristic interdigitation of the root with vocalic morphemes appears to have no counterpart in PIE.

I shall quote PA reconstructions about as I reconstructed them before my article on 'Formosan evidence for some new proto-Austronesian phonemes' (*Lingua* 14.285–305 [1965]). The attempt to make new citations would involve irrelevant difficulties; the chief unfavorable consequence of considering the Formosan material is the substitution of members of the *S set (*S_1, *S_2, etc.) for *h, as in PA DewS_3a 'two' for former *Dewha (36) and PA buS_{14}ek (i.e. either *S_1 or *S_4) for former *buhek (68).

PIE reconstructions are cited without prejudice in the orthography used

by C. D. Buck in his *Dictionary of Selected Synonyms in the Principal Indo-European Languages* (1949). I have been somewhat lax in citing supporting forms for PIE reconstructions only because of the type of audience. I use either PA for Proto-Austronesian or PH for Proto-Hesperonesian before reconstructions based on Austronesian comparisons according to applicability. In many cases a given comparison contains only members of Western Malayo-Polynesian languages, which fall under a hypothesis of a Hesperonesian subgroup. Since some may regard marking such etymologies with PA as confusing or even as tending to overrate the evidence, I have followed the practice of distinguishing between PA and PH etymologies for an audience presumably mainly made up of IE scholars. There is no intention of taking the audience in; on the other hand, it will be difficult to avoid all pitfalls in attempting to make an objective presentation.

The 'Evidence'

Among the pronouns we find (1) PA ia '3rd sg. pronoun', which can be compared with PIE *eyo- *eyā- (as in Latin *eō*, *ea*) beside *i- (e.g. Latin *i-s*). Undoubtedly this same element is to be found in the Malay relative particle *ya-ŋ* 'that', in which the second element is originally the 'linker' found in many Philippine languages [e.g. Tagalog *malaki ŋ ba·hay* 'large (linker) house'].

A striking similarity is to be found in (2) PA *aku* 'I' (Tag. *qako*, Samoan *aʔu* 'I') when compared with PIE *eĝh- eĝ-, especially in view of the Ilocano enclitic *-ak* 'I', though the loss of vowel may be analogical in origin. It must, however, be admitted that there is no other instance of PA k || (*read* 'matches') PIE ĝ or ĝh.

Another example is the element (3) *yu which appears in Tagalog *ka-yo* (Ilocano *ka-yu*) 'ye' which contains the morpheme *ka-* that also appears in Tag., Ilc. *ka-mi* 'we (exclusive)'. Although we should like to compare this directly with PIE *yu- 'ye' we should inevitably face difficulties because *yu is perhaps limited to Philippine languages in this meaning. The more widespread forms are *ka-mu (Malay, Toba Batak, Fiji) and *ka-miu [Chamorro, Nggela, and possibly reflected in Tagalog *qi-nyu* 'you (oblique)' with an assimilative replacement of *m by *n*, perhaps originally in the form *n-inyo* 'of you, your (pl.)']. The second element of *ka-mu* resembles the pervasive *mu 'of thee, thy' and it is therefore conceivable that Tagalog *ka-yo* 'ye' is a replacement of an earlier *ka-mu with *qiyo/yo* 'thee (oblique), thy' in place of *-mu. [It is also possible, but less plausible, that a more original form was *ka-nyo.] In any case, there is no easy solution for Ilocano *kayu*; it may be due to borrowing or a common inheritance (with Tagalog *kayo*) from a common proto-language where the replacement of *kamu took place. It is to be noted that the Cordilleran (North Luzon) languages that exhibit forms of *ka-yu have *yu 'your (pl.)'.

The form *miu, quite common in Melanesian languages, is probably to be assigned to PA. It is found also on Luzon in the Baler Negrito enclitic *miu* 'your (pl.)' and with metathesis in the Casiguran, Casiguran Negrito, and

Cagayan enclitic *moy* 'your (pl.)'. Regardless of what the relation between *ka-mu and *ka-miu is, the simplest hypothesis will thus place them both in PA; *ka-mu could have originated as an adaptation of *ka-miu under the influence of *mu, and *ka-miu could possibly be a dialectal replacement of the *-mu of an earlier universal *ka-mu with a second plural possessive element *miu. The evidence for an original PA *yu is thus not strong. But if out of the PA forms a morpheme *yu 'you (pl.)' could develop, it is not difficult to imagine it happening in another daughter language of the putative PAIE.

PA clearly has (3) *mi in *ka-mi 'we (exclusive)' and this can be associated, perhaps with some difficulty, with PIE *n̥sme- 'we'.

One is tempted to compare (4) PA kaw 'thou' (inexplicably associated as it is with the unexplained relation between Javanese *kowé* and Futuna *koe* 'thou') with PIE *tewe- *twe- *te-. However, thus far PA k || PIE t has not turned up elsewhere.

Among the demonstratives (5) *i-(tT)u (Tag. *qito*, Merina *itu* 'this', Malay *itu* 'that') suggests PIE *to-, and (6) *i-ni (Tag. *d-i·ni* 'here', Ml. *ini* 'this', Merina, Ulawa *ini* 'that') suggests perhaps the source of Skt. *e-na-* 'he', Lat. *oi-no-s* 'one' or is perhaps to be associated with PIE *eno- *ono- 'this, he'.

It is attractive to compare (7) PA apa (Ml. *apa*, Fi. *θ-ava* 'what'), with PIE *k^{w}o-, especially if one considers at the same time the association of (8) PA epat (Tag. *qa·pat*, Toba Batak *opat*, Fiji *va* 'four') with PIE *q^{w}et^{w}or- under some hypothesis of the loss of what follows the *t. That eventually we may have to reconstruct a PA *epati is suggested by Saa. *hai* '4' < *pati.

There are other indications that PA has labials for the labiovelars of PIE. Thus for example one can compare (9) PA beRat (Ml. *berat* 'heavy') with PIE *g^{w}r̥u-, g^{w}ru- 'heavy' and at the same time (10) PA bei, b-in-ei 'woman, wife' (Ml. *bini* 'wife', Tonga *fe-fine* 'woman, girl') with PIE *g^{w}enā- and (11) PA bunuq, Tag. *bunoq* 'war', Toba *bunu*, Mal. *bunoh*, Mer. *vuno* 'kill', Saa. *hunu* 'slaughter' with PIE *gh^{w}en- 'strike, kill'.

Before going on with the matchings for PIE labiovelars, it is in place to cite other instances of PA R || PIE r in connection with the association of PA beRat || PIE *gh^{w}r̥/ru-:

(12) PA waiR, Mal. *aer*, Fiji *wai* 'water'.
PIE *wer-, Skt. *vār-*, *vāri* 'water'

(13) PA (dD)aRat, Tag. *da·gat* 'open sea', Mal., Toba *darat* 'mainland'.
PIE *ter-s-, Lat. *terra*, cf. also Ir. *tīr* 'land'.

(14) PA uRat, Mal. *urat*, Ngaju *uhat*, Fiji *ua*, Tonga *uo-ua* 'sinew, vein'; Tag. *qugat*, Toba *urat* 'root, sinew, vein', West Nakanai *la-ua* 'root'.
PIE *wr̥̄d-, *wr̥d-, Lat. *rādīx* 'root', Gt. *waurts* 'root'.

One would not compare additionally (15) PA Ramut, Bisayan *gamut*, Ilc. *ramut*, Baranago *lamu* 'root' with Lat. *rāmu-s* 'branch' if the latter is from *rād-mo, but perhaps this etymology is not inevitable.

Perhaps in connection with the PIE labiovelars one might with some

hesitation also suggest comparing (16) PA putiq 'white' (Mal. *puteh*) with PIE *ǩweit-, Skt. *śveta-* 'white' with a change analogous to the one in the labiovelars.

But not all PIE labiovelars match PA labials. In (17) PA tuma 'louse' compared with PIE *kʷr̥mi- 'worm' and in (18) PA qatey 'liver' compared with PIE *yĕkʷr/n we find PA t || PIE kʷ. Perhaps with these is to be associated the matching of (19) PA tulaŋ, Mal. tulaŋ 'bone' with Lith. *káulas* 'bone', Lat. *caulis* 'stalk'.

At the same time PA p || PIE p. For example (20) PA apuy, Tag. *qapoy*, Mal. *api* 'fire' can be compared with PIE *pur- pun- pewor- with PIE r/n for PA y as in the words for 'liver' (18) above. Similarly (21) PH padaŋ 'field' (Tag. *pa·raŋ*, Mal. *padaŋ*) can be compared with Gk. *pédon* 'field'.

To return to the numerals, we can compare (22) PA esa, Tag. *qisa* 'one' with PIE *sem 'one'. At the same time we can compare the following which show PA s || PIE s:

(23) PA sepsep 'suck' (Tag. *sipsip*, Toba-Batak *sopsop* 'suck').
PIE *seup-, Skt. *sūpa-ḥ* 'broth, soup', OHG *suvil*, *sufili* 'a sip'.

(24) PA susu 'breast' 'Ml. *susu*, Tonga *huhu* 'woman's breast').
OCS *sŭsŭ* 'breast' (also currently assigned to PIE *sup-s but perhaps not inevitably).

(25) PH sikil (Jav. *sikel* 'foot, leg', Isneg *si·kil* 'foot').
Gk. *skélo-s* 'leg'.

(26) PA sakit 'sick, hurt' (Tag. *sa·kit*, Tonga *ma-haki* 'sick').
Gt. *siuk-s* 'sick', Lith. *saugus* 'cautious'. [For PA k || PIE g, cf. (27) PA takub (Tag. *takob* 'cover', Fiji, Futuna *taku* 'tortoise shell') with PIE *(s)teg.]

(28) PA nusa, Jav. *nusò*, Mer. *nusi* 'island', Sa'a *enute* 'name of Florida Island'.
Gk. *nē̃sos* 'island'.

(29) PA asin 'salty', Tag. *qasin* 'salt', Tonga *m-ahi* 'sour'.
PIE *sal- 'salt'.

Likewise (30) PA sapu 'wipe, sweep' is perhaps best matched with PIE *swep-, sweb- (ON *sōfl* 'broom, *sōpa* 'sweep', OCS *sypati* 'strew, scatter'), but on the contrary perhaps with Lat. *scōpae* 'broom' (lit. 'twigs'); consider then the possible association of (31) sinaR 'light' (Tag. *si·nag*, Mal. *sinar* 'ray of light', Fiji *θina* 'torch') with PIE *skī-n- (Gt. *skeinan*, OCS *sinąti*, etc. 'shine').

In connection with the proposition that PA n || PIE l, as in PA asin || PIE *sal (29 above), one can cite:

(32) PA penuq, Mal. *penoh*, Tonga *fonu* 'full'.
PIE *plē- (but possibly pl̥-no-), Gk. *plērē-s*, Lat. *plē-nu-s* 'full'.

(33) PA Daun, Dawen, Mal. *daon*, Jav. *ròṇ-ḍòn*, Mer. *ravină*, Tonga *lau* 'leaf'.
PIE *dhăl- 'bloom, flourish', Gk. *θáll-ō* 'bloom, flourish', Ir. *duille* 'leaf', Alb. *dal* 'sprout', and perhaps Lat. *folium* 'leaf'.

(34) PA taneq, Mal. *tanah*, Toba *tano*, Mer. *tani*, Saa *ano* 'earth, land'.

PIE *tel- 'flat land', Skt. *tala-* 'surface, bottom, plain', Lat. *tellus* 'earth'.

(35) PA *Dewha, Bis. *duha*, Mal. *dua* 'two' is to be associated with PIE *dwō(u), *dwai, etc., 'two'. Similarly, (36) PH (dD)ilaq, Tag. *dilaq*, Mer. *lela* 'tongue' can be associated with PIE *dnĝhwa-; at the same time another instance of PA l || PIE ĝh could be found in (37) PH Zilap, Zilat, Ngaju *jelap* 'play with tongue', Mer. *lelaf-ină* 'to be licked', Mal. *jilat*, Toba, Jav. *dilat* 'lick', if associated with PIE *leiĝh- 'lick'. However the last is the only instance thus far of PA Z || PIE l.

At the same time a different matching of PIE ĝh appears in the following:

(38) PA miR, mi(R)miR, Tag. *halu-migmig* 'dampness', Toba *mimir* 'gush forth', Fiji *mimi* 'squirt', *mi* 'urinate', Saa, Tonga *mimi* 'urinate'.
PIE *meiĝh-, Gk. *omeíxō*, Lat. *mingere* 'urinate'.

(39) PA abaRa, Toba *abara*, Ngaju *baha*, Saa *ahala-*, *hala-* 'shoulder'.
PIE *bhāĝhu-, Skt. *bāhu-* 'shoulder'.

(40) PA telu, Toba *tolu*, Mer. *telu*, Fiji, Tonga *tolu*, Saa *olu* 'three' lends itself to association with PIE *tri- 'three'. Other instances of PA t || PIE t have already appeared. Another instance of PA l || PIE r appears in:

(41) PA teluR, Tag. *qi-tlog*, Ml. *telor*, Jav. (literary) *antelu*, Saa *sa-olu* 'egg'.
Rus.-ChSl. *jadro* 'kernel, testicle', Pol. *jądro* 'kernel', pl. *jądra* 'testicles', perhaps also Skt. *aṇḍa* (? < *andra-) 'egg, testicle'.

(42) PH salay, Toba *sale*, Mel. *salay*, Mer. *sali* 'to smoke', Jav. *salé* 'dry in sun'.
Lat. *serēscere*, OHG *serawēn* 'become dry', and perhaps Gk. *kserós* 'dry'.

(43) PH saluR, Tag. *saʔog* 'stream', Mal. *salor* 'conduit'.
PIE *sreu- 'flow'.

Still another possibility appears in:

(44) PA kulit, Ml. *kulit*, Fiji *kuli-* 'skin'.
PIE *(s)ker- 'cut' as in Lat. *corium* 'hide, leather'. [However association is possible instead with PIE *(s)keu- 'cover' or PIE *(s)ken 'cut off'.]

And for PA t || PIE d, cf. (45) PA quti, Tsou *kutʔi*, Paiwan *kuti*, Puyuma *koti*, Bunun *kutte* 'vagina', Ngaju *uti*, Fiji *uti* 'penis' with PIE *udero-, wedero-, e.g. Skt. *udara-* 'belly'. [The PA cognates could however also be compared with Gt. qipus 'womb, stomach' and its cognates.]

It is possible only with difficulty to associate (46) PA pitu, Tag. *pito*, Jav. *pitu* 'seven' with PIE *septṃ 'seven' without some explanation for the absence of a matching in PA for the PIE initial sequence se-. A similar absence of PA initial s appears in the matching of (47) PA inep, imp-i, ip-i 'sleep, dream', Tag. *panag-inip* 'dream', Ml. *inap* 'spend the night', Saa *eno* 'lie down'; Ml. *m-impi*, Jav. *impi* 'dream'; Toba, Jav. *ipi* 'dream' with PIE *swep-no- 'sleep'.

It is tempting to associate (48) PH paqit 'bitter' with Gt. *baitrs* 'bitter'.

As another instance of the matching of PA p with PIE bh one could compare (49) PA quDip, Jav. *udip*, Futuna *ma-uli* 'live' with Gt. *liban* 'to live'. In this connection another instance of the matching of PA D with PIE l can be added: (50) PA tu/iDuR, Ml. *tidor*, Ngaju *tiroh*, Tag. *tulog*, Mer. *turi*, Saa *me-uru* 'sleep', cf. OIr. *con-tulim* 'sleep' and also OCS *toliti* 'pacify, quiet'.

Despite the many other PIE matchings for PA R, there is still another that can be added, namely PIE l. Consider the following:

(51) PA bibiR, Tag. *bibig* 'mouth', Toba *bibir*, Mal. *biber* 'lips', Fiji *mbembe* 'vulva' (considered as a reduplication of the monosyllable *biR).
Ir. *bēl* 'lip'.

(52) PA baReq, Tag. *bagaq* 'abscess', Toba *baro*, Mal. *barah*, Jav. *wòh*, Mer. *vai* 'swelling, abscess', Trukese *pwo* 'swell, be pregnant'.
"PIE *bhlō, *bhlē-, extension of *bhel- in words for 'leaf' . . . and perhaps originally 'swell'." [Buck, 526]. Cf. Lat. *flōs*, etc.

A striking matching is offered by (53) PH e(m)bun, Tag. *ambon* 'drizzle', Toba *ombun* 'cloud', Mal. *ĕmbon* 'dew' and PIE *nebh-, *n̥bh-* in words for 'cloud' (> 'sky'), 'mist', 'shower' (Buck 65), Skt. *nabhas-* 'mist, vapor', *abhra-* 'cloud', Gk. *ómbros*, Lat. *imber* 'shower'. For PA b || PIE bh we can add:

(54) PA belaq, Toba *bola*, Mal. *bĕlah*, Fiji *mbola* 'split'.
PIE *bheid-, Skt. *bhid-*, Lat. *findere* 'split'.

though thus far another instance of PA l || PIE d has not appeared (and perhaps PA belaq is to be compared with Lat. *frangere*, Gt. *brikan* 'to break').

We can further compare (55) PA biŋkuk, Mal. *beŋkoq*, Ngaju *biŋkok* 'bent, curved', Tonga *piko* 'bent' with PIE *bheug(h)- in Skt. *bhuj-* and Gt. *(ga)biugan* 'bend'. (We have seen above two instances [PA *sakit (27) *takub (28)] of PA k || PIE g.) Still another example of PA b || PIE bh is perhaps to be found in:

(56) PA buhat, Tag. *bu·hat* 'raised high', Toba *buat* 'taken away', Mal. *buat* 'do, make, build', Mer. *vuatră* 'arrange', Fiji *vuat-a*, Futuna *fuat-a* 'harvest'.
Gt., OE *būan* 'dwell, inhabit', OHG *būan* 'dwell, inhabit, till', ON *būa* 'dwell, arrange, prepare', Dan. *bygge*, Sw. *bygga* 'settle, found, build'.

Although PA b has already been found to match other PIE reconstructions, there is evidence for PAb || PIE w:

(57) PH balu, Toba, Mal. *balu* 'widowed'.
PIE *widhewā- 'widow' (from PIE *weidh- 'separate').

(58) PA bilit, Ngaju *bilit* 'wrap around', Samoan *fili* 'be wrapped up'.
PIE *wer- 'cover, shut'.

(59) PA baseq, Tag. *basaq*, Mal. *basah*, Ngaju *bisa* (metathesis) 'wet', Toba *baso* 'watery', Fiji *sava* 'wash'.
PIE *wed-, Skt. *ud-* 'wet, flow', Gk. *húdōr*, Gt. *watō*, etc. 'water'.

For PA s for PIE d, as in the last example above, cf. (60) PA ma-les, Toba *malos* 'wither', Mal. *malas* 'lazy', Saa *malo* 'asthma' with Gt. *lat-s*, etc. 'lazy', Lat. *lassus* (*ləd-to-) 'tired, weary', OCS *lěnŭ* (*lēd-no-) 'lazy'.

Not only does PA s match PIE d as above, but also PIE t:

(61) PA serep, Toba *sorop* 'nettle', Mal. *serap*, Jav. *serep* 'press in', Saa *toro* 'bore through'.
PIE *ter-, Gk. *tetraínō* 'bore', *trētós* 'perforated', etc., and extended in Gk. *trūpáō* 'bore', Lith. *trupėti* 'break into pieces'.

(62) PA sulam, Tag. *sulam* 'embroidered cloth', Mal., Jav. *sulam* 'embroider', Fiji *θula* 'bore through', *i-θula* 'needle'.
PIE *(a)teu-d, Skt. *tundate* 'push, sting, prick', etc.

(63) PA su(ŋ)kit, Tag. *suŋkit* 'forked pole, hairpin', Mal. *suŋkit* 'pick teeth', Ngaju *suŋkit* 'embroidery', Mer. *sukitră* 'work with a sharp instrument', Fiji *θukit-a* 'dig with a stick', *i-θuki* 'digging stick', Samoan *su?i* 'to thread, embroider'.
PIE *(s)teu-k-, OCS *tŭknąti* 'prick, strike', *istŭknąti* 'dig out', Gk. *túko-s* 'hammer, chisel, battleax', Welsh *twll* 'perforated', or
PIE *(s)teu-g-, Skt. *tujáti*, *tójati*, *tuñjáti*, *tunakti* 'press, push, strike, impel, OHG *stoc* 'stick, staff, trunk', etc.

There is some indication that PA p also matches PIE w in:

(64) PH e(m)pu, Tag. *qimpo* 'grandmother', *qapo* 'grandchild', *qapoq* 'grandfather', Toba *oppu* 'grandparents', Ngaju *empu* 'parents-in-law', Tag. *poq* 'sir', Fiji *vu-ni-wai* '(lord of water) doctor'.
PIE *awo-, Lat. *avus* 'grandfather'.

(65) PA pules, Toba *pulos*, Mal. *pulas*, Jav. *pules*, Ngaju *pules*, Mer. *fuli*, Fiji *vuloθ-a* 'twist', Saa *hulo* 'wrap up', *pulos-i* 'turn something around'.
PIE *wert- 'turn', but perhaps rather PIE *wel-d- 'roll'.

It may also be that (66) PA panaq 'bow, arrow' is to be connected with Skt. *vāna* 'arrow, reed' (?with *n̥* < *ln, cf. Gt. *walu-s* 'staff', ON *val-r* 'round').

PA b appears to match PIE p in the following:

(67) PA batu, Tag. *bato*, Mal. *batu*, Fiji *vatu* 'stone'.
Gk. *pétra* 'rock'.

(68) PA buhek, Tag. *buhok*, Toba *obuk* 'hair', Tonga *fuk-a* 'cut hair'.
Gk *pókos* 'fleece, wool', OE *feax*, OHG *fahs* 'hair'.

A possible matching of PA j appears in the following:

(69) PA (?)aji, Mal. *adiq*, Fiji *t-aθi*, Sam. *t-ei* 'younger sibling'; and *(?)anji*, Toba *aŋgi*, Ngaju *andi* 'younger sibling'.
PIE *dhi- 'suck', cf. Lat. *fīlius* 'son'.

(70) PA qajaw, Tag. *qa·raw* (<*qa-l-jaw) 'day', Buginese *ɛ̂sso*, Saa *s-ato* 'sun', Tonga *aho*, Fut., Sam. *aso* 'day'; and *qa (n) jaw, Ngaju *andaw*, Mer. *andru*, Ibanag *aggaw* 'day', Fut. Sam. *ao* 'day, light'.
PIE *dyeu-, diw-, etc. 'day'.

The following also seem appealing:

(71) PA kali, Toba *hali* 'dig', Ngaju *kali* 'pit', Mer. *hadi*, Saa *ʔeli*, Sam. *ʔeli* 'dig'.
PIE *kel- 'strike', Lat. *per-cellere* 'strike', Gk. *kláō* 'break', etc.

(72) PH mulut 'mouth', *mulot* 'mouth', Mer. *mulutrǎ* 'lips'.
OHG *mūla* 'mouth (of animals)'; or possibly Gt. *munþ-s*, etc. 'mouth', (?) Lat. *mentum* 'chin'.

(73) Balinese *anaq muani*, Sawu *moṉe*, Trukese *mwään* 'man'.
Skt. *manu-*, Gt. *manna*, etc. 'man'.

The following, though appealing, are of much more doubtful value:

(74) Pujuma *soan* 'dog'.
PIE *k̂won- 'dog'.

(75) PA Rumaq, Toba *ruma*, Mal. *rumah*, Ngaju *huma*, Saa *lume* 'house'.
Gt., etc., *rūm* 'space, place', ME *roume*, NE *room*, Sw. *rum* 'room (in house)'.

In addition to the preceding we may add two bits of morphological evidence. The first of these is the (76) PA nasal infix which is placed before the second consonant of a root. A number of doublet roots have resulted: e.g. PA tubuq, Tag. *tuboq*, Toba *tubu*, Jav. *tuwoh* 'grow'; PA tumbuq, Ml. *tumboh* 'growth', Ngaju *tumbo* 'sprout'.

There is some indication that this infix is connected with verb formation: cf. Ml. *tuboh* 'body' with *tumboh* 'grow'. This infix could be connected with the present formative nasal infix in PIE: cf. Skt. *yukta-ḥ* 'joined', *yuñj-maḥ* 'we join'.

The (77) PA suffix -en, which forms words meaning 'something which undergoes such and such an action' [Tag. *ka·n-in* 'boiled rice' (*-ka·qin* 'eat'), Mal. *makan-an* 'food' (*makan* 'eat'), Futuna *kan-o* 'meat' (*ka-i* 'eat' < *kaen 'eat + -i)], can be compared with the IE group of formants with n exemplified in Gt. *bit-ans* 'bitten', OCS *nes-enŭ* 'carried'.

Finally (78) the durative reduplication of Austronesian languages [Tag. *nagpu·pu·tol* 'is, was cutting' (*nagpu·tol* 'did cut'), if this reduplication can be connected with that of Trukese *mömmööt* 'be sitting', *mööt* 'sit'] can be compared at least with reduplication in the formation of present stems in PIE (e.g. in Gk. *títhēmi* 'I put') and perhaps in other formations.

Conclusions

This completes the presentation of the evidence thus far gathered. One could proceed to examine what kind of a proto-language is implied, but under the conditions there is no pressing need to do so. Suffice it however to say that the number of comparisons could be increased at least slightly, perhaps even substantially, without a severe loss of quality. On the other hand, further research in Proto-Austronesian may ultimately cut down the number of attractive comparisons. I have aimed to avoid obviously evitable matchings and will be very happy to receive suggestions for corrections.

I must confess that I am impressed with the extent to which I have been successful in gathering matchings between the reconstructions of the two

families. Granting that thus far I have not wavered in my steadfast belief in the likelihood that an Austronesian-Indo-European relationship can not be demonstrated, should I waver now?

Notes

[1] This article contains results obtained with the support of NSF Grant 23824.

[2] Joseph H. Greenberg, *Essays in Linguistics* (Chicago, 1957) 43.

[3] P. Benedict, Thai Kadai, and Indonesian: A new alignment in Southeastern Asia. *American Anthropologist* 44.576-601 (1942).